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Article I.—*On Dropsy.* *By* D. Francis Condie, M.D.

Among those diseases, the pathology of which is still involved in great obscurity, may be ranked dropsy. From his total ignorance of, or at best, his imperfect acquaintance with, the physiology of the exhalent and absorbent systems, it was not to be expected that the physician should have been able to form very correct views of the nature of their morbid conditions ; hence if we examine the various explanations which have been given of the formation of dropsical effusions, from the earliest periods of our science to a very late date, we shall find them, with few exceptions, to be mere vague hypotheses, presented without argument, and unsupported by a single fact. To enumerate the various opinions which have been entertained in relation to this subject, would indeed be a tiresome and useless task, —waiving it therefore, we shall enter at once upon what we believe to be a more interesting and profitable inquiry,—an inquiry into the morbid condition of the several tissues and organs of the body, with which the production of dropsy would appear to be intimately connected ;—for it is a fact, which every candid reasoner will be obliged to admit, that dropsy is more frequently to be viewed as the symptom of a disease, than as the disease itself.

By almost every medical writer of the present period, dropsy is supposed to result from an increased secretion, or a diminished absorption, of those fluids which are furnished, during a state of health, by the serous and analogous tissues. The proposition would, perhaps, be more correctly stated, were it said that dropsy depended upon a want of balance between the exhalation and absorption of the serous fluid of the cavities;—for although the increased exhalation is rendered evident by every symptom of the disease, the diminished absorption is by no means so clearly proved,—and that dropsy ever depends solely upon a diminished activity of the absorbents is a conclusion we cannot admit. That, in dropsy, absorption does not, in the affected cavity, equal in extent the amount of exhalation, is acknowledged; this must be the case, or the accumulation of fluid constituting the disease, could never take place;—but that, in dropsy, absorption is ever to a less extent than in the normal condition, has, as yet, never been attempted to be proved. That the action of the absorbents, generally, is not impaired, we know, from the fact that interstitial absorption goes on, in many cases, even to a greater extent than during health, as is evinced by the very great emaciation which is exhibited in those parts unaffected by œdema. Mercury and other remedies administered by friction, are absorbed in this as readily as in other diseases. Blood when effused beneath the cuticle is also removed by the absorbents with equal facility as under ordinary circumstances;—again, one serous cavity may be alone affected with accumulation, while in every other the exalation and absorption shall be in perfect equilibrum,—and even while every other part of the body is distended by a morbid collection of fluid, absorption will be found to go on, with perfect regularity, within the cavities of the joints, the synovial sheaths, the tunica vaginalis testis, &c. The general absorbent system, we may therefore infer, is not, during dropsy, in a state of diminished activity.

The only arguments which we have ever heard adduced in support of a diminished absorption in this disease, are drawn from the diminished exhalation from the skin, and the paucity of urine, two symptoms almost invariably attendant upon dropsy. Neither, however, have any connection with the circumstance they are brought forward to prove; neither the dryness of the skin, nor the diminished quantity of fluid discharged by the kidneys, has any relation to a diminished absorption;—both facts are to be explained by the augmented secretion which takes place from the surface of the serous membrane.

The external and internal exhalations are, in some measure, antagonists to each other; when the one is increased, the other being most generally diminished. This is strongly exemplified in the exhalations of the skin and those of the bowels, which are always in inverse proportion to each other; the same, by a parity of reasoning, we may presume to take place between the cutaneous exhalation and that of the serous membanes, particularly when the latter is in a morbid condition.

In regard to the small quantity of urine passed during the existence of dropsy, we are to remark that the amount of this excretion is, invariably, in proportion to the amount of fluids required to be removed from the circulating mass; and that whenever these fluids are discharged by any other outlet, whether by increased exhalation from the surface of the body, or by an augmented secretion into the intestines, the urine is always in diminished quantity:—when we consider, therefore, the amount of fluid poured out, in dropsy, by the serous membrane, we can very readily account for a diminution in the urinary discharge. "Urina idcirco parcè redditur," says SYDENHAM, "quia sanguinis serum, quod per ductus urinarios ex lege naturæ excerni debuerat, jam in cavitatem abdominis deponitur, &c*."

It is to be recollected, however, that, even during the existence of dropsy, we have no reason to presume the saline matters secreted by the kidneys, to be less in amount than under ordinary circumstances.

The rapidity with which the fluid reaccumulates, in cases of ascites, after tapping, is another argument against the supposed origin of dropsy from diminished absorption. In the case of a female, labouring under ascites, which came under our notice; from the 24th of February 1825 to the middle of October of the ensuing year, the operation of paracentesis was performed thirty-five times, and the fluid drawn off amounted to one hundred and eighty-six gallons:—the usual interval between the operations was two weeks, and the quantity discharged each time, about five gallons. It is impossible to believe that so great an amount of fluid could be produced, in so short a period, by the ordinary action of the exhalents, even if we were to suppose the absorbents of the cavity to be totally inactive.

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* Tractat. de Podag. et Hydrop. p. 70.

From what has been said, we may, I think, safely set out with the proposition, that dropsy depends upon an augmented exhalation of the fluids furnished by the serous and analogous tissues ;—so far, clear and positive facts bear us out, but whether a morbid condition of the absorbents ever concurs in the production of the disease, we have as yet no means of judging.

The next question that presents itself is, upon what does this increased exhalation depend ? Every one who is conversant with the writings of the British physicians, is well aware that according to the pathological views of a majority of them, even to the present day, dropsy is considered to be a "disease of debility." What is exactly to be understood by a disease of debility, it is not very easy to determine,—the expression itself conveying no definite idea ;—that, however, it is employed by these writers to indicate a condition entirely opposed to the existence of inflammation, or of increased action of any kind,—something which entirely precludes the use of depletion, we are well aware, from the fact of their either forbidding the employment of the lancet and the abstraction of blood in any manner, or recommending it with all the precaution requisite in the most dangerous experiment,—restricting its use to relieving certain distressing or immediately dangerous symptoms, and scarcely ever admitting it as a part of the curative plan. In accordance with the genuine spirit of this doctrine, Dr Good remarks, upon the plan of treating dropsy by purgatives and diuretics, that "it should be regarded as a preparatory step only ; a mere palliative ; and an evil in itself, though an evil of a less kind to surmount an evil of a greater."—"Every purgative that does not diminish the general bulk, adding to the general disease by increasing the debility*."

If by denominating dropsy a disease of debility, nothing more were intended than that, in many cases, it is accompanied by a paleness of the surface, diminished activity of the voluntary muscles, and a diminished action of the heart and arteries, while we reprobated the admission of such laxity of expression into pathological language, we should have no objection to acknowledge, that to a certain extent the statement is true ; and we shall hereafter attempt to shew that the condition of the circulation and of the muscles referred to, may, in some instances, be even a cause of dropsical effusion. We

* Study of Medicine, Fourth Edition, Vol. V. p. 385.

apprehend, however, that what most of those who define dropsy to be a disease of debility mean by the expression, is that it owes its origin to a laxity, weakness, or diminished tone of the exhalents concerned in its production,—or as a late writer has explained the theory, "to a relaxed state of these vessels, which *open their mouths too widely*, and suffer the serum or other aqueous fluid to escape with too much freedom*."

The advocates of such a doctrine, must certainly suppose the serous fluid, which lubricates the various cavities, to be separated mechanically from the blood,—that it oozes, as it were, through orifices which are so adapted in size as only to admit a certain quantity to pass, until these orifices are enlarged by disease. Such views of exhalation cannot, however, be admitted in the present state of physiology. The function of the exhalents is to furnish a fluid of a particular kind, adapted in quantity to answer certain purposes;—when, therefore, we find this fluid furnished in an augmented quantity, the conclusion is evident,—the exhalents have been increased in activity. If the serous fluid given out by the exhalents was accumulated in certain reservoirs, and the exhalent orifices acted merely as sphincters to prevent its discharge excepting under certain circumstances, or to a certain amount, we should then understand in what manner their relaxation, their *gaping open*, would allow an increased discharge of serum to take place. But viewed in the light of organs adapted to separate from the blood a certain product, let them at any time sink into a state of weakness, or of relaxation, and the only effect that could result would be a diminution of that function, never its augmentation. We therefore conclude, that in every case of dropsy there is an increase of activity in the exhalents. But, as has already been hinted, this increase of activity is, in almost every instance, an effect of previous disease of one or more of the tissues or organs of the body, upon the removal of which, when practicable, the cure of the dropsy depends. Hence, it will be perceived, that, in a practical point of view, a consideration of the several diseases productive of dropsical effusions, is a subject of very considerable interest; in relation to it, therefore, we propose in the present essay to offer a few general remarks. And first of inflammation.

An increased serous effusion is the result of inflammation. Al-

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* Good, Study of Medicine, *loc. cit.*

though this is not laid down as a universal proposition, to which no exceptions can be adduced, yet it will be found, upon examination, to be more universally true than is commonly supposed.

Laennec has shewn that inflammation of the pleura is invariably accompanied by an increased effusion of serosity into the cavity of the chest, and the same can be proved to be the fact in relation to every inflammation occurring in the serous tissue. The larger joints, the cavities of which are lined with a membrane almost identical with the serous, seldom, if ever, become inflamed, without being distended by a morbid serous effusion; the same increased secretion of serum is found in inflammations of the tunica vaginalis testis, of the pericardium, &c. It is indeed surprising, that this dependence of dropsy upon inflammation should not, until of late years, have more generally attracted the attention of physicians; particularly when they had, every day, under their notice the fact of an increased effusion of serum, resulting from the inflammation of a blister, of erysipelas, of violent friction applied to the skin, of burns and scalds, &c. They were probably, however, not aware that the effusion in these latter cases is precisely the same as that constituting dropsy.

To Dr Rush is certainly due the credit of having first pointed out to the physicians of our own country the intimate connection between certain dropsies and inflammation; and of founding upon this pathology a more rational and successful practice than that previously pursued. But he was by no means the first medical writer who was aware of this fact. Monro had, previously, inculcated the necessity of direct depletion, in all cases of dropsy attended with the same general symptoms as were then supposed to be the only indications of an inflammatory disease. Stoll, likewise, in his "*Prælectiones in Diversos Morbos*," published in 1788, describes an inflammatory dropsy, to be removed by antiphlogistic and emollient remedies. The same fact appears, indeed, to have been noticed from the earliest periods of medical history, as we infer from the circumstance of blood-letting having been considered an essential remedy in certain cases of dropsy, by Hippocrates (*de Dieta in Acutis*), by Galen, Alexander of Trailles, Paul of Eginetus, Riverius, Botallus, Etmuller, Mead and others. The frequent connexion between dropsy and inflammation is, likewise, confirmed by a reference to numerous cases, with their examination after death, recorded by Morgagni and Lieutaud; and to the works of Van

Sweiten, which contain frequent examples of dropsy occurring during or immediately subsequent to many of the phlegmasiæ.

But it is almost entirely from the writings of physicians of a very late period, that any very definite facts can be collected in support of this point. Previously, even by Dr Rush, dropsy was spoken of in a very vague manner, as depending upon, or as connected with an inflammatory, or phlogistic condition of the system, and although the treatment founded upon opinions, thus loosely expressed, was, in its general outlines, correct, yet it could, by no means, be so well adapted to individual cases, or so generally successful, as when, from a closer attention to the symptoms of the disease, in connection with the facts derived from morbid anatomy, we become better acquainted with the seat, nature and extent of the inflammation by which it is produced, and are enabled, in consequence, to apply and modify our remedies accordingly. The same objection of vagueness which has been attributed to the language of Rush, and of preceding and contemporary writers, on the subject of the inflammatory character of dropsy, may be applied, with nearly equal propriety, to that of Dr Parry, who was among the most zealous of the modern defenders of this doctrine in Great Britain.

Cruickshank, though, comparatively, an old writer, appears to have been well acquainted with the connexion which subsists between dropsical effusion, and inflammation of the serous membrane. "The second species of dropsy," he remarks "is very common;—it is that which arises in consequence of previous inflammation of a cavity (those of course lined by the serous tissue), and may take place in any habit of body. If an inflammation take place in a cavity, it may terminate in a number of different ways; one of these is by an increased secretion of the fluid of surfaces. A man receives a blow on the testicle, inflammation takes place, and the consequence is, frequently, a hydrocele, or dropsy of the tunica vaginalis. A child's brain inflames, and this inflammation ends at last in hydrocephalus, or collection of water in the brain. In like manner pleurisy frequently terminates in hydrothorax, or collection of water in the chest. I have often," he adds, "taken away forty to sixty pints of water, which had accumulated in the cavity of the abdomen, in the few days the peritoneal inflammation had lasted, during the usual species of child-bed fever. This is to be considered as substituting a less dangerous disease for another;—peritoneal inflammation kil-

ling often in three days, while ascites may last for twenty years*." Precisely the same opinion, in regard to the formation of abdominal dropsy, was maintained by PEMBERTON, who viewed it as a species of natural cure of peritonitis; his words are, "sometimes a resolution of the inflammation takes place, when ascites is produced†." From his morbid anatomy, we find that Dr BAILLIE, likewise, held similar views in regard to the formation of hydrothorax.

LAENNEC, whose name is in itself a host, speaking of dropsy of the chest, after having noticed an increased secretion of serum as an invariable effect of pleuritis, pericarditis, &c. remarks, that "whatever may be the difference, both in the general symptoms and the organic lesion, between a case of hydrothorax and an acute pleurisy, or between a case of ascites from general debility or organic disease of the heart or liver, and the same disease from an attack of peritonitis; or in short, whatever may be the difference, in general, between a dropsy and an inflammation,—there can be no doubt that these two affections, so opposite in their extreme degrees, are, nevertheless, often very nearly allied in their slighter shades:—we frequently find albuminous concretions amid the serum of ascites, and purely serous effusions in inflammatory affections of the chest‡."

Acute dropsy of the pericardium, says ITARD, like all other acute dropsies, is almost invariably the result of an inflammation, and consequently is almost always symptomatic. It is more especially an inflamed condition of the pericardium, or of a neighbouring portion of the pleura or lung, that we are to regard as its most ordinary cause§. The general dependence of this variety of dropsy upon pericarditis had been previously insisted upon by PINEL.

So well convinced was HUNTER of the connexion of dropsical effusion with an inflamed condition of the vessels of a part, that he has enumerated, among the varieties of inflammation, one which he denominates the œdematous; the distinguishing symptom of which is an extravasation of serum into the cellular membrane at the part affected‖. A similar species of inflammation is described by JADELOT,

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* Anatomy of the Absorbent Vessels, Second Ed. 4to, 1790.

† Diseases of Abdominal Viscera, Fourth Ed. p. 57.

‡ De l' Auscultation Mediate, &c. Tom. I.

§ Sur les Hydropisies, Paris, 1818.

‖ Treatise on the Blood, &c. Philad. Ed. p. 238.

in his work on the diseases occurring in the Hospital for Sick Children at Paris.

That hydrocephalus owes its origin to inflammation of the membranes of the brain we have the most conclusive evidence. "Nothing more occurs in this disease," observes Dr RUSH, "than what takes place when hydrothorax follows inflammation of the lungs, or where serous effusion follows inflammation of the joints."—In one hundred and seven individuals who fell victims to an inflammation of the membranes of the brain, observe MM. PARENT and MARTINET, in a late work*, the brains of sixty-seven presented, upon examination, an effusion of serum to a greater or less amount.

The foregoing facts and opinions are fully confirmed by the experience of every medical writer, who has presented us with a pathological history of inflammation of the serous membranes, based upon morbid anatomy.

To be convinced of the inflammatory origin of dropsy, it is only necessary, indeed, to examine after death the cavity in which the morbid accumulation is seated. Thus in hydrothorax and ascites, the pleura and peritoneum will, in very many instances, be found not only highly vascular, but deprived of their natural transparency and glossy appearance, and covered, to a greater or less extent, with patches of coagulable lymph. Occasionally, it is true, so great an interval may have elapsed between the existence of the inflammation and the fatal event, as to render it somewhat difficult to recognize the more decided marks of its previous existence; but even in such cases, the thickening of the serous tissue, and the presence of portions of false membrane adhering to the surface of the cavity, or of shreds of coagulable lymph floating in the effused fluid will indicate, with certainty, that inflammation must have existed. It is very correctly remarked by PORTAL, that a post mortem examination has frequently shown the existence of inflammation in cavities affected with dropsical accumulation, even when no such inflammation has been indicated during the life of the patient, or, at least, its symptoms have been so obscure as to have escaped the attention of the medical attendant†.

The following conclusions, the correctness of which is established

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* Sur l' Inflammation des Meninges, &c.

† Observations sur la Nat. et le Trait. de l'Hydrop. Tom. I. p. 62.

by the result of numerous pathological observations, very readily explain the formation of what have been termed acute dropsies.

Whenever a serous membrane is attacked with acute inflammation, the action of its exhalents becomes altered, and in place of the almost imperceptible halitus which lubricates the surface of the membrane during a state of health, they secrete an albuminous fluid, which, immediately upon its being discharged from the vessels, separates into two distinct parts. The one solid, answering to the coagulable lymph of HUNTER, and the other fluid. The solid portion adheres, for the most part, to the surface of the membrane, and becoming, subsequently, penetrated with blood-vessels, constitutes what have been termed false membranes and the medium by which morbid adhesions are produced between the contiguous parts of a cavity;—while the fluid portion is, in general, taken up by the absorbents as quickly as it is formed. The above constitutes, in fact, the process by which a natural resolution of most acute inflammations of the serous membrane is effected. In certain cases, however, it happens that the serous portion of the morbid secretion is so great in amount, that the absorbents are incompetent to its immediate removal; a portion of it remains of course in the cavity in which the inflammation is seated, and constitutes the foundation of a dropsical accumulation, which latter never fails to be fully formed, provided the inflammation upon which it depends be not speedily and effectually removed.

The amount of serous fluid furnished by the exhalents in phlegmasiæ of the serous membrane, would appear, very generally, to be in proportion to the extent of surface occupied by the disease, and, also, to the slowness with which the inflammation runs through its several stages. When the inflammation extends to a small portion of the membrane only, and runs a rapid course, then effusion of coagulable lymph, or adhesions between contiguous surfaces is the most common result,—but when the disease occupies nearly the whole surface of a cavity, and runs more slowly through its stages, we have an abundant secretion of a serous fluid, while but little coagulable lymph is effused.

The foregoing remarks are more particularly applicable to inflammation of the pleura and of the peritoneum. When the serous membranes of the brain become inflamed, the effect is, generally, a morbid secretion of a limpid fluid, containing so small a portion of albuminous matter, as scarcely to undergo any degree of coagula-

tion upon the addition of an acid, or by exposure to heat. It is certain that in many cases of arachnitis coagulable lymph is effused to a very considerable extent,—but such is by no means so common an effect of inflammation of this membrane as the increased serous discharge alluded to.

From what has preceded, we find, therefore, that in every instance in which inflammation is seated in the serous tissue, there is more or less tendency to the formation of a dropsical accumulation; and that STOLL was perfectly justifiable, when, in his aphorisms, he placed dropsy among the several terminations of the internal phlegmasiæ.

The frequent dependence of dropsy upon inflammation might be still further established by an examination into the external causes to which it succeeds, the symptoms by which, in many cases, it is accompanied, and the treatment most effectual in its removal;—but what has been already said, we conceive to be sufficient upon this point. The fact which is so very generally admitted by the physicians of this country, is daily gaining credit among the profession in Europe. It has been maintained in Great Britain by PARRY, WELLS, BLACKALL, AYRE, and the Edinburgh school generally;—in France by DUPUYTREN, MARENDELL, ITARD, BRESCHET, PORTAL, &c. and in Italy by GEROMINI and others.

Our remarks, heretofore, have been almost exclusively confined to acute inflammation; but dropsical accumulations are still more frequently the result of inflammation in its chronic form. This variety of disease, but little understood, and too often entirely overlooked, by physicians, before they were made acquainted with its true character and symptoms by the writings of BROUSSAIS, in perhaps the majority of instances, when seated in the serous tissue, terminates in a morbid effusion within one of the great cavities. The histories of chronic pleuritis and peritonitis, as detailed by LAENNEC and BROUSSAIS, will sufficiently evidence this fact.

Ovarian dropsy we believe to be, in every instance, the result of a chronic inflammation of the substance of the ovary, which after continuing for a longer or shorter period, extends finally to its serous envelope. This is likewise the opinion of Dr AYRE, who has recently published a very valuable treatise upon dropsy. It is to be remarked, that in cases of ovarian dropsy, the fluid discharged by puncturing the diseased organ, is never of so serous and limpid a character as that contained in the abdomen in ordinary cases of ascites; but is either a thick glareous matter of various colours, or

a turbid fluid, containing portions of a membranous appearance, pus, albuminous floculi, &c. and further, that the disease never continues for any length of time without producing an effusion of serum within the cavity of the abdomen. In every instance of ovarian dropsy which has come under our notice, the effects of extensive inflammation of the peritoneum were exhibited, upon examination after death.

Dropsy from chronic inflammation is too often the consequence of mismanagement on the part of the practitioner:—an acute affection of the thorax or abdomen, treated by improper remedies, or without sufficient vigour, in its first stages, may be deprived of its acute character, but still continue, masked under a chronic form, to produce the most extensive ravages,—the more fatal from their true character being unsuspected. This is the source of very many of those dropsies which succeed to violent febrile affections:—dropsies which have been ascribed, almost invariably, to an exhaustion of the constitution from the preceding disease.

We find, says PORTAL, in his treatise on dropsy already quoted, in the writings of MORGAGNI, SENAC, LIEUTAUD and other celebrated authors, such frequent examples of dropsy succeeding to fever, whether continued, remittent or intermittent; they are indeed of so common occurrence; that we should be always upon our guard to prevent their formation, by the prompt and entire removal of the febrile affection; for the success of our remedies in these dropsies, after they have taken place, is always exceedingly doubtful. On opening the bodies of those who die, he adds, in the cases here alluded to, we are presented with engorgements, obstructions, and even suppurations of the viscera. In another part of the same work, he remarks, in relation to dropsy succeeding to an inflammation, that so far from an abuse of blood-letting giving rise to it, as has been supposed by some, it is frequently to be referred to the circumstance of the evacuation of blood not having been carried to a sufficient extent, in consequence of which, an engorgement of the inflamed organ is permitted to remain, or to take place, at the very time when the patient is supposed to be upon the recovery. The truth of this important remark might be verified by a host of examples.

Although we are inclined to refer the production of dropsy, in a majority of instances, to pre-existing inflammation, yet we are persuaded that every case cannot be traced to a similar source.

Among the causes of dropsy independent of inflammation, we may

mention an impediment to the free circulation of the blood, particularly in the veins. By consulting the records of our profession, we shall find it was long believed, that whatever prevented the free return of the blood to the heart would occasion a morbid effusion of serum into the cellular membrane, or one or other of the great cavities;—lately, however, the fact has been as positively denied, as it was before strenuously contended for.

One of the principal evidences in proof of serous effusion as a result of obstructed circulation, has been drawn from the experiments of LOWER. This gentleman, towards the middle of the seventeenth century, finding that a serous infiltration occurred in the extremity, upon the upper part of which a ligature had been applied, was induced to ascertain whether the same effect would result when one of the principal veins of the body was tied. He accordingly applied a ligature upon the vena cava of a dog, immediately below the diaphragm, and found that the abdomen and posterior extremities became dropsical. In another experiment, when the two jugulars were included in a ligature, an effusion of serum was very rapidly produced in the cellular membrane of the face and neck of the animal; and in a third, hydrothorax was produced by a ligature of the vena cava, immediately before it enters the right auricle. Notwithstanding these experiments are liable to many objections, they were urged by BOERHAAVE as conclusive, and we are informed, that they were repeated by BONETUS with every precaution, and with a similar result. That the conclusions drawn from them correspond with a variety of pathological facts we have no hesitation in asserting. Take an individual, the compositor, for example, in a printing office, who is obliged, for the greater part of every day, to remain standing in one position, and we shall find, especially when from intemperate habits, or other cause, the tone of his general system has been reduced, that very soon he will be affected with œdema of the feet and ancles, which, at first, disappears during the horizontal posture assumed at night, but subsequently becomes permanent and gradually ascends to the legs. In this case we have several causes tending to impede the venous circulation in the lower extremities,—the effect of gravity upon the ascending column of blood, from the constant erect position of the body,—the want of muscular motion, &c. We know also, that individuals, thus circumstanced, are precisely those in whom a varicose distention of the veins of the legs is of most frequent occurrence.

There is, in fact, scarce a person, who has passed the meridian of life, unless he possess an unusual share of constitutional vigour, who is not, towards the close of the day, affected with an œdematous swelling of the feet, to a greater or less extent, and a very evident over distention of the veins of these parts. Few old people, it is observed by PORTAL, can be found who do not labour under œdema of the legs:—the same author notices, also, the frequency of these swellings in individuals who make long journeys in a carriage;—in both instances, he adds, they are occasioned by the impediment the blood experiences in its return to the heart; they very readily disappear upon the patient's assuming the horizontal position at night; and by proper exercise during the day, are prevented from returning*.

It may, however, be objected, that in the instances above referred to, the impediment is not confined to the passage of the blood in the veins, but extends equally to the contents of the lymphatics. This objection, although it does not appear to us to be of much weight, may be met by a reference to those cases in which the impediment is confined strictly to the circulation of the blood. Thus, hydrothorax is produced by malconformations of the heart, &c. Numerous observations collected by MORGAGNI, BONETUS, LIEUTAUD and others, had already demonstrated the intimate dependence of morbid effusion within the chest upon aneurismal swellings of the ventricles or auricles of the heart, polypus concretions within its cavities, augmented size of its parietes, &c. but it is more particularly to the researches of CORVISART and subsequent writers, that we are indebted for any clear information on this subject. From these authorities we learn, that the derangement of the circulation, arising from organic lesions of the heart, the slightest in appearance, as for example, ossification of one or more of its valves, or even a simple disproportion between the capacity of its cavities, occasions, in almost every instance, a dropsy of the thorax†. The same effect is produced by a varicose dilatation, or an obstruction, compression or obliteration of the pulmonary veins, consequent upon certain chronic affections of the lungs. Aneurism of the aorta, malconformations of the thorax, &c. are, likewise, well known causes of hydrothorax.

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* Op. citat. Tom I. p. 204.

† Sur les Maladies et les Lésions Organiques du Cœur, &c.

But cases have been lately recorded, in which the obstruction is still more strictly confined to the venous circulation. I allude to those instances, in which, from inflammation of the lining membrane of a vein its cavity is obliterated, or the circulation through it is impeded in consequence of the formation of coagula. The cases of Davis, in the Medico-Chirurgical Transactions, are of this character; for although the relator has denominated them instances of plegmasia dolens, they were evidently nothing else than anasarcous swellings of the lower extremities, resulting from obliteration of one or more of the large venous trunks, in consequence of preceding inflammation. This conclusion is confirmed by the phenomena of a case related in the 13th volume of the last mentioned work by Dr Forbes. A journeyman printer, twenty-six years of age, had the whole of the left lower extremity enlarged to double its natural size;—the swelling extended from the foot to the groin;—it every where pitted upon pressure, presenting an appearance perfectly similar to that observed in common anasarca. The sub-cutaneous veins were distinctly marked, being distended with blood. The patient died; and on examining the diseased limb, the cellular tissue was found, over its whole surface, very much distended with a limpid fluid; the veins were tense and filled with coagulated blood;—the superior portion of the femoral vein, as well as the external and common iliacs, were found filled with a firm coagulum having much of the fibrous appearance of the coagula contained in aneurismal sacs:—on separating the coagulum, a thin but distinct membranous layer was seen adhering to the internal coat of the vein, requiring some force to separate it. Morgagni relates, also, the case of a female whose left lower extremity was œdematous, in which, after death, he found the left lower iliac vein filled with a coagulum, as he terms it, of a polypus nature, the canal being entirely obliterated by it; while the crural vein was at least one third narrower than natural, and appeared to have been in that state for some considerable time. (*Epis.* lvi. *Sec.* 10.) Another case is recorded by the same author (*Epis.* l. *Sec.* 55) of a somewhat similar character.

In the body of a man who had been affected with œdema of both the lower extremities, Raikem found the inferior cava, above the emulgents, filled with a concrete, fibrous matter of a whitish colour. In another instance of dropsy of the lower extremities, but particularly of the right, recorded by the same writer, the inferior cava, towards its bifurcation, and the left primitive iliac were filled with

a reddish matter, analogous to the fibrine of the blood somewhat altered, while the right primitive iliac was impervious, being converted into a ligamentous chord.

In the Archives Generales, Tom. II. and IV. Bouillaud has collected a great variety of facts upon this point. In one instance to which he refers, an œdematous swelling of the right side of the face and neck was found to be connected with an obliteration of the internal jugular, and of all the venous branches which terminate in the right subclavian :—in another, in which an anasarca, at first confined to the upper extremities, extended, subsequently, over the whole body, the two cavæ were almost entirely obliterated by coagula of a fibrous appearance : in two other instances referred to by M. Bouillaud, ascites resulted from an obliteration of the vena portæ ; the cavity of the vein, in the one, being filled with a solid fibrous coagulum, in the other, by a coagulum of a fibrous appearance, but of a pulpy consistency.

In a case reported by Mr Travers, after the operation for popliteal aneurism, in which the femoral vein, as well as the artery, was tied, a serous infiltration of the leg quickly ensued*. Mr Hodgson, likewise, relates an instance of dropsy of the lower extremities from obliteration of the femoral vein, while Rhodius refers to an instance of ascites, in which the descending cava was so much contracted, as scarcely to allow the passage of a middle sized probe into its cavity.

Every affection of the lungs by which the circulation through them is rendered difficult, is also attended with morbid effusion within the chest. Thus in cases of hepatization, schirrhus, tubercles, fungus hæmatodes, melanosis, and various other affections of these organs, more or less of a serous fluid is invariably effused into the cavity of the pleura, and the death of the patient is, frequently, preceded by the most urgent symptoms of hydrothorax, while upon examination after death the pleura exhibits no symptoms of an inflammation having existed, to which the formation of the serous accumulation could be attributed.

The foregoing facts are, we conceive, sufficient to prove that a congestion of the capillaries, consequent upon an impeded circulation through the returning vessels of a part, is capable of rousing the exhalents to an increased secretion. But it is proper here to remark, that morbid effusion of a serous fluid is not a *necessary*

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* Cooper and Travers's Surgical Essays, p. 179.

and *invariable* consequence of the passage through a principal vein being suspended;—thus, Mr Hodgson reports a case in which the femoral vein was tied, and Mr Simmons of Manchester, another, in which the same operation was performed upon the internal jugular*, and in neither instance, we are informed, was any degree of dropsical effusion produced. Rayer and Bricheteau tied, in rabbits, the abdominal cava, above the origin of the emulgents, the primitive jugulars, the axillary veins, &c. and, but in one instance, found that the impediment to the circulation thus produced, gave rise to any considerable degree of serous effusion†. Travers reports, upon the authority of Wilson, a remarkable case, in which, not only the whole extent of the vena cava, above the origin of the hepatic veins, but also the emulgents, the spermatics, the primitive iliacs and their principal branches, were found filled with lymph and coagula of considerable size, without any appearance of serous effusion having resulted, in the limbs or other parts of the body‡, and Dr Baillie, in his observations on the obliteration of vessels, relates a somewhat similar case§. It is from analogous facts that Hodgson concludes dropsy very rarely to result from the obliteration of a principal vein‖, which is also the opinion of Beclard; while Bichat, who, as we know, refers every disease to an alteration in the vital condition of some portion of the system, has strongly combated the supposition, that dropsy, in any instance, arises from a mechanical, or other impediment to the free course of the venous blood. To this sweeping conclusion, we cannot, however, subscribe; we have already referred to instances of serous effusions intimately connected with the very cause here denied ever to produce them, and many more might be cited. During pregnancy, particularly in its advanced stages, what is more common than an œdema of the lower extremities? and to what cause can we ascribe this latter, other than the pressure exerted upon the vessels within the pelvis, by the pregnant uterus? The condition of the veins of the extremities is a sufficient proof, that, under the circumstances alluded to, the circulation through them is impeded. It is stated by Rayer, in evidence of the œdema

* Medical Facts and Observations, Vol. VIII. p. 23.
† Sur l' Hydropisie. Paris, 1825.
‡ Vide Medico-Chirurg. Trans. Vol. III. p. 65.
§ Ibid. Vol. I. p. 127.
‖ Vide Diseases of Arteries and Veins.

in these cases resulting from the pressure of the gravid uterus upon the vessels, that when there occurs during pregnancy a lateral obliquity of the uterus, the œdema is confined to the side corresponding with that to which the uterus is turned.

Œdema of the feet and legs is likewise an effect of the pressure from aneurismal tumours seated in the ham or groin; while an axillary aneurism, in like manner, occasions an œdematous swelling of the hand and arm of the side upon which it is seated.

In cases of scirrhus or cancer of the female mammæ, when the glands of the axilla become enlarged, the entire arm of the affected side becomes most generally anasarcous. In lumbar abscess, also, one of the very first symptoms is, not unfrequently, an œdema of the extremity of that side upon which the collection of matter is seated, which continues until the abscess finding its way to the surface no longer compresses the vessels of the limb. The same is equally true of most deep seated collections of matter:—in these an œdema being, for a long time, the only symptom of their existence.

"Other circumstances," says RAYER, "which relax the course of the venous blood through the vessels of the extremities, give rise frequently to œdema. Thus, a varicose condition of the superficial or deep seated veins of the leg, is many times accompanied with a watery intumescence of the feet and ancles: examples of this have been collected by M. BOULLAUD, and," he adds, "there is not perhaps a single physician who has not observed the same."

It was remarked by RUYSCH, that a varicose condition of the spermatic veins, very commonly precedes the formation of hydrocele. MORGAGNI likewise (*Epis.* xliii. *Sec.* 34) was of opinion that the latter disease may be produced by the existence of an obstacle to the free return of the blood from the testicle:—certain it is, remarks POTT, that the spermatic veins are very often found varicose in persons afflicted with hydrocele of the tunica vaginalis; while, according to RAYER, the greater frequency of hydrocele of the left side is, undoubtedly, connected with the fact of varicocele occurring in a majority of cases, in the veins of that side.

The last mentioned writer notices, likewise, the circumstance of inguinal and crural herniæ producing, frequently, œdema of the foot and leg, by the pressure which they exert, when they have attained to any considerable size, upon the vessels of the limb. This, according to RICHTER, is more commonly the case in omental hernia, and is attributed by him to the pressure upon the vessels from a portion

of hardened omentum being greater than that produced by a hernial sac containing only intestine. A case is referred to by HANNEMANN, of a female labouring under omental hernia, with extensive œdema of the foot, which had continued without abatement for many months, but quickly disappeared after the reduction of the hernia.

Œdematous swellings are also very commonly symptoms attendant upon extensive tumours, the result of inflammation, but of an indolent character, situated in the neck, axilla, groin, ham, upon the leg, &c.* In some of these cases, however, we apprehend that the morbid effusion of serum is not to be ascribed, solely to compression of the venous trunks, it being more than probable that the extension of irritation to the surrounding cellular membrane may concur in causing the increased action of the exhalents.

Is abdominal dropsy ever produced by an obstruction to the portal circulation, arising from a diseased condition of the liver? This is admitted by a great majority of the writers on dropsy; but by BICHAT and others, is replied to with an absolute negative. BICHAT, in particular, maintains, that the obstruction here supposed cannot possibly take place, in consequence of the free communication kept up between the two cavæ by means of the vena azygos†; and Dr AYRE who, in his late very valuable treatise, refers all dropsical effusions, in every instance, to a particular grade of inflammation, asserts, that we have no satisfactory instances yet shown to us, of such a precise condition of the liver as has been supposed to interrupt the freedom of the circulation within the abdomen‡. "There are," he indeed admits, "numerous instances of abdominal dropsies, in those labouring under a scirrhous or enlarged state of the liver; but there are, also, numerous examples of such states of the liver, as well as of the spleen and the other organs, without any such effu-

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* I have seen, says PORTAL, in a man, in whom the parotid and maxillary glands of the right side were swollen and hardened, the whole of the right arm become œdematous. In another case, after an inflammatory swelling of the right parotid, there occurred an œdematous intumescence of the head and neck, at first only on the side with the inflamed parotid, but subsequently extending to the opposite side also;—the œdema passed gradually to the superior part of the chest, and from thence even to the abdomen. The inflamed gland finally suppurated and discharged a considerable quantity of purulent matter, after which the œdema quickly disappeared. *Op. citat.* Tom. I. p. 120.

† Anatomie Generale, Tom. I. p. 397.

‡ Vide Researches into the Nature and Treatment of Dropsy, &c. London, 1825

sion; and in many cases, when such effusion has taken place, it has been carried off by the natural passages, or by tapping, without any return of the dropsy;—and yet without any visible change in the structural condition of the liver*."

"I think," observes Dr AYRE, in another part of his work, "it may be assumed that ascites, when proceeding from visceral disease, (and the same principle applies to hydropic effusions from pressure of disease in other cavities) does so by the gradual extension of the chronic inflammation of the internal cellular or serous tissues of the diseased organ, to its outer or external coverings; and that, commencing here as from a point, the serous or hydropic inflammation is progressively propagated throughout the whole of the serous membrane of the cavity."

Although we are much pleased with the above explanation of the cause of dropsy subsequent to disease of the liver and other viscera, and believe that it will be found applicable to very many of those cases, yet we can by no means admit it to be true to the extent to which the author would wish to carry it. There is no doubt in our minds, that in not a few instances, one of the chief causes giving rise to dropsies, connected with certain diseased conditions of the liver, is an interruption to the free circulation through the abdominal veins;—the morbid condition of the organ, as discovered after death, allowing no room for the suspicion of a chronic or hydropic inflammation having been communicated from its substance to its peritoneal covering. In many cases of ascites it is evident that the blood circulates much more slowly, or in a much less quantity, through the liver, than under the ordinary circumstances of this viscus; it being found, after death, reduced to a hard, compact mass, from the vessels of which, on its being cut into, scarce a drop of blood escapes:—in other, and perhaps the greater number of cases, we have, however, an impediment to the circulation of a very different nature; there being congestion, with an overloaded state of the vessels of the liver, and of all the branches of the vena portæ, —a condition of things which, we are fully persuaded, whenever it

* We hardly think this statement will be borne out by the general experience of physicians:—that the liver may be diseased without producing ascites we acknowledge; but when abdominal dropsy is connected with an organic affection of that organ, so long as this affection exists, we hazard the assertion, that no permanent cure of the dropsy can be effected.

is not speedily removed, occasions invariably a morbid effusion of serum, to a greater or less amount.

Dr Ayre is forced, from the weight of facts, very reluctantly to admit that pressure upon the vessels of a limb will occasion an œdema below the spot at which the pressure exists.—"The remote cause is here," he remarks, "indeed of a mechanical kind, but not so the proximate cause of the effusion. By the resistance given to the blood's return by the principal veins of the limb, a reaction is occasioned in the extremities of the arteries leading into the corresponding extreme branches of the veins, which reaction is in this, as in a multitude of other occasions of congestive fulness in these vessels, a sanative effort of nature to overcome the primary obstruction." It is merely the fact of the obstruction, that we are now contending for;—to Dr Ayre's explanation of the mode in which the dropsy is produced in these cases we have no objection;—it is the one which we ourselves should advocate, if not in the exact form in which he has expressed it, at least in substance.

To the class of dropsies arising from diminished activity of the venous circulation, we refer, without hesitation, those infiltrations of the extremities consequent upon great prostration of the system. Take, for example, an individual during the period of convalescence from a violent febrile affection, or one who has recently experienced an excessive hemorrhage; no sooner does he rise from his bed and attempt the slightest exercise, than more or less of œdema will very generally occur in the feet and ancles. The venous blood, in such cases, accumulates in the vessels of the inferior extremities. In consequence of the languid and inefficient action of the heart and general circulation, and the prostration of the muscular system, it is not acted upon by a sufficient force to overcome entirely the effects of gravity which it experiences in the erect posture of the body. No sooner, however, has the action of the heart acquired its healthy vigour,—the muscles increased in tone,—and the patient enabled to deliver himself to a proper degree of exercise, than the intumescence of the feet and legs entirely disappears. It was a deficiency in the moving powers of the venous blood that produced it; —that deficiency being supplied, and the increased effusion upon which the œdema depended no longer continuing, the superabundance of serum accumulated in the cellular membrane is quickly removed by the absorbents, and the healthy condition of the parts is restored. But we must be cautious, not to confound with the par-

tial dropsies just alluded to, those which occur subsequent to cases of violent disease, in which, as was stated in a former part of this essay, the morbid action has never been entirely subverted, but merely converted from an acute into a chronic form. Almost all the cases of dropsy collected by PORTAL, and supposed by him to have been occasioned by exhaustion from previous disease, are of this character, as is proved by a reference to post mortem examinations;—the facts revealed by which are strangely overlooked by the author.

Upon the above facts and arguments, we would lay down the two principal causes productive of dropsical effusion to be, 1st, Inflammation, either acute or chronic, of the serous or analogous tissues, and 2d, An impediment to the free circulation of the blood through the veins. Other causes of less frequent occurrence, or of a more equivocal character might be cited, but the space we have already occupied in a very brief and superficial examination of the facts adduced in reference to the two first, will prevent us, at present, from noticing the latter.

ARTICLE II.—*Observations on Pruritus Vulvæ.* *By* JOHN RUAN, M.D. *Fellow of the College of Physicians.*

[Read before the College, 27 May 1828.]

I have been so little in the habit of committing my thoughts to writing, that I should be wasting the time of the College, if I were to attempt a regular essay or dissertation,—but that I may comply with their requisition in the best manner I can, I will give a short statement of a few cases that have fallen under my notice, of a disease about which I believe very little has been published. I have been many years in practice, yet I never met with a serious case of pruritus vulvæ until the year 1822: the first case which engaged much of my attention occurred to me early in that year. The patient had been lately married, but did not become pregnant for some time after. She first complained of strangury, for which the usual remedies were resorted to, and at length was attacked with such an itching about the pudenda as made her miserable. Purging with vari-

ous articles, diuretics, drinking largely of mild diluting drinks, with a free use of opiates and rest, procured some mitigation of her sufferings, but nothing like a cure was effected until the balsam copaiva was used. This, in doses of twenty drops three times a day, acted like a charm, and she was completely relieved. She has two or three times since had slight attacks of strangury, for which the balsam has been used with immediate advantage. I did not meet with another case of this kind until June 1825. In this instance the patient was far advanced in pregnancy, and the strangury was by no means an urgent symptom; but the itching was so intolerable, that although a delicate young woman, she could scarcely refrain from scratching in my presence. It did not occur to me to use the balsam, until I had tried bleeding, purging, diuretics and opiates freely and repeatedly, together with various applications to the part. She also washed the parts with lead water, and with borax dissolved in water; it was applied finely powdered. Lime water, warm milk and water, and poultices of bread and milk were tried. Indeed I may say, every thing I could think of, or that was suggested to me by some of my medical friends, was resorted to without any advantage. I had some how lost sight of the balsam; but at length it was brought to my recollection, and again answered perfectly. My patient was cured by it directly, and I now considered myself in possession of a very important, if not an infallible remedy for a distressing disease. But the very next case, which occurred in September 1826, showed the fallacy of this opinion. This woman, like the last, was near her confinement. The balsam failed entirely, but she was relieved by washing with a solution of borax. My fourth case was that of a lady about seven or eight weeks after delivery. Her confinement had been protracted for such a length of time by various complaints, and she was at last attacked by this very troublesome one, which set at nought all the remedies I had before made use of; but she was relieved by the carbonate of soda in small repeated doses. My fifth patient was delivered in July last; she was a woman of delicate health, suffered from a tedious labour, and did not receive all the attentions her situation called for; she had repeated attacks of fever during her confinement, and in the third week this distressing complaint came on with strangury, which called for the repeated use of the catheter. In addition to the remedies that had been tried in the preceding cases, an infusion of the common laurel was used as a wash, at the suggestion of a medical friend, but without effect. She

could be kept in tolerable comfort only by the constant application of bread and milk poultices and large doses of laudanum. I now had recourse to the use of the lapis calaminaris, and had the part dusted with it, but she complained of this producing smarting and pain. Being desirous of trying it further, I mixed it with an equal quantity of powdered starch. She now bore it without complaining, and the itching gradually subsided. She continued the use of it for a week or two. This was the only case in which the parts were inspected by me. The swelling about the meatus urinarius was so great, that I was obliged to use a very small catheter, and the difficulty of passing this gave me a fair pretext for this examination, which discovered no other mark of disease than what might be attributed to the violent scratching.

Having given this short account of these cases, I refrain from any observation upon them, but shall be much gratified if I succeed in drawing information from my fellow members.

ARTICLE III.—*An Inquiry into the State of the Biliary Secretion in Fever. By* WILLIAM WOODSON WADDELL, M.D. *of Athens, Georgia.*

The arguments advanced in the following essay are founded on an opinion, opposed to the general belief, that the fevers of the southern states are bilious in their character. In proving this position, the author will be obliged to contend against popular prejudice, rather than medical authority properly so called; for it may be remarked, that it is the mass of the community who believe the doctrine, and not scientific physicians. Many professional men, no doubt, have fallen in with the prevailing opinion, that redundancy or depravity of bile exercises much influence in our fevers; but something has hitherto prevented them from accounting for or proving this position. In essays on fever, the subject is occasionally introduced, but, if we may judge from appearances, it has seemed to claim attention, only in consequence of the universal belief in the powerful agency of bile, and not from any conviction in the minds of the authors, that this agency was correctly imputed

to it. It is not pretended, that the arguments here brought forward are altogether the result of observation; but they will not be condemned by the enlightened reader, because the proofs have not always been derived through the medium of the senses. Our senses are known to be imperfect; and in reasoning on subjects that require their minute employment in so great a degree as physiology and pathology, we arrive at a point where they totally fail to instruct. Here we draw on the resources of our reasoning powers; for it will be readily admitted, that many things are demonstrable to reason that are not to sense: reference to a principle in natural philosophy will convince us of the truth of this position. In endeavouring to detect the ultimate particles of matter by the aid of the microscope, they become so minute as to be confounded with the surface on which they are placed: at this point, the powers of the organ of vision assisted by the resources of art no longer avail; but reason goes many steps farther, and satisfies us that matter is infinitely divisible.

Hitherto, whenever the agency of the bile in fever has been doubted, the author of such opinion has been accused of mistaking cause for effect. To free the present essay from that imputation, three positions will be assumed.

1. That redundancy or depravity of bile is not the cause of fever.

2. That neither are the necessary effects of fever.

3. That neither are directly or indirectly the effect of the acknowledged causes of fever.

In considering the first position, we shall investigate the physiological effect of the usual quantity of healthy bile on the system, and thence form an opinion of the probable pathological influence of an increased quantity, or morbid quality of the same fluid. We are taught that it is the office of one portion of the bile to assist in the separation of the chyle from the chymous mass brought into the duodenum, and that the office of another portion is to stimulate the alimentary canal and cause it to propel its contents. The reality of the latter office is now a settled point in physiology. That bile contains a purgative principle has been proved by the experiments of professor CHAPMAN, who found that the bile of inferior animals was actively cathartic. Negative evidence to the same effect is derived from the fact, that in those diseases, where external symptoms indicate a deficiency of biliary secretion, there is always torpor

of the intestines and obstinate costiveness. This is exemplified in jaundice, dyspepsia and several other diseases. Having these opposite effects according as the secretion is healthy or deficient, the natural inference would be, that a redundancy of the secretion would purge or produce diarrhœa. The same effects would result from acrimony or depravity of the bile. This state of the biliary secretion has been attributed to the influence of solar heat, occasioning an abstraction of the more watery parts which it contains. It is to be presumed that the bile, subjected to such a process, must acquire a concentrated power, and that its irritating and corrosive effect on the mucous membrane would be greater according to the intensity of the irritation. We should therefore expect either a violent diarrhœa, or a mild or severe dysentery. If then bile, in a state of redundancy or depravity, be the cause of our fevers, we have a right to expect that those fevers would be preceded or accompanied by diarrhœa or dysentery. But these affections, so far from being common, are unusual attendants on fever. Is it not, therefore, probable, that our fevers, called bilious, are in fact never caused by an altered state of the biliary secretion?

The second position assumed is, that redundancy or depravity of the bile is not the effect of fever. It would be transcending the prescribed limits of the present essay to attempt an exposition of the effects of all the phenomena of fever on the biliary organs. We may, however, take one of the most common, namely, universal oppression, followed by arterial reaction. It is agreed on all hands, that in the first mentioned state, there is a general diminution of secretion. It will also be conceded, that when reaction and arterial excitement take place, the secretory vessels will return to the execution of their proper functions in most of the glands. But the liver is known to be an exception to this rule, since it derives the materials of its secretion from its veins instead of its arteries. By the augmented action of the arteries, the veins become engorged with blood, and being destitute of any power of reaction, sufficient to overcome this morbid distention and obstruction, it follows that circulation will be partially, and secretion totally suspended.

But the most satisfactory view that can be taken of the subject, will be, to establish the third position assumed, or in other words to trace the common causes of fever in their influence on the animal economy, and ascertain whether they can be so modified in their application to the system, as to be productive of increased or depraved

biliary secretion. This will in a great measure include all that it would be necessary to say in a separate consideration of the symptoms; for if we follow the causes to their ultimate effects, we shall certainly, at some stage of our progress, encounter the more prominent symptoms. Among the causes of fever, marsh effluvium presents itself as most important. The manner in which it enters the system is yet a contested point. By some it is believed, that it acts on the skin, that it paralyses the capillary vessels, and that the circulation, being thus obstructed on the surface, is greatly augmented in the deep seated vessels. The latter then make an effort to disburthen themselves of their load, but owing to their overpowered and debilitated state, they are unable to succeed, and the fruitless effort is called fever. Suppose we admit this theory to be true: the veins, and especially those that supply the larger viscera, would accordingly become engorged from two sources: they would receive their proportionate quantity of the blood confined to the internal parts by the paralysing effect of the miasmata on the surface, and they would have an unusual quantity forced into them by the increase of arterial action which always takes place in fever. As they are not capable, from their organization, of transmitting their contents in a ratio to be compared to the arteries, the engorgement will necessarily continue. Is it to be supposed then, that those veins, from which are derived materials for the secretion of bile, can in their disabled state furnish elements for the production of an unusual amount of that fluid. We are taught by physiologists, that the blood, before it arrives at a gland, must undergo some preparatory change, which predisposes it to furnish the elements for the peculiar secretion for which it is destined. But can this preparatory change take place, when the blood is forced so rapidly into the veins of the liver by the power of arterial excitement? And if it does not take place before the blood arrives at the place of secretion in the liver, can we expect it to occur after that time? In the latter case, the original ability of the blood to effect those changes, or its former susceptibility of having such changes produced in it, is most probably lost, because it has become morbid in character by having assumed an unnatural stationary position. It is necessary to the healthy condition of the blood that it should be decarbonized by the oxygen in the lungs; but this change cannot be effected in blood which is stopped in its course by engorgement in the vessels of a large gland, and thus prevented from passing through the lungs. Physiology

recognises no other point at which the blood can come in contact with oxygen, and consequently carbon must remain in the engorged blood and thereby become diseased. It will of course no longer furnish the necessary pabulum for secretion.

A new theory of the modus operandi of miasma was published in Charleston in 1820. By this theory, it was supposed that it entered the system through the medium of the function of respiration; that being a combination of noxious gases, it acted only by virtue of its mass or bulk, by which, in a mechanical manner, it prevented the necessary quantity of oxygen from entering the lungs. That portion of carbon which is accumulated in the blood in the round of the circulation, being thus detained in it, was supposed to produce the same effect on the blood as so much milk, ether, alcohol, or any foreign substance, injected directly into the blood vessels. Now if this theory be true, the blood would be in a *positive* state of disease; but a healthy condition of the blood is requisite to supply the proper materials for secretion, and in its diseased state there will not be an increase but a diminution of this function.

It is believed by one class of pathologists, that miasmata affect the system through the medium of the olfactory nerves, and that their action is transmitted to the stomach by means of the intimate connexion existing between all parts of the mucous membrane. The nervous expansion of this tissue would, of course, be the principal agent in conveying the poisonous impression. But can we suppose that the nerves, in the performance of this office, will remain free from disease themselves? Can they be the messengers which carry death and disorder to every other part of the system, and yet be unaffected in their own delicate structure? Certainly not; for if marsh miasma has any existence, it must diminish, and possibly paralyse, the energy of the nerves when applied to them. It is a beautiful doctrine in physiology which teaches us, that secretion depends for its immediate cause on an intuitive perception of the nerves, by which they discover that the materials for secretion are passing along in the blood vessels and are ready to be drawn out. As soon as this perception is made and communicated to the proximate secerning agents, they immediately act and the work of secretion takes place. Rejecting the idea of any mechanical agency in a process so purely vital, and assuming the nervous influence as the efficient cause of secretion, it becomes evident that any interruption of that influence must cause a corresponding derangement in the action of the secre-

ting power of the glands. Now according to this theory of the modus operandi of miasmata, the energy of that part of the nervous system, which is subservient to the secretion of bile, is directly diminished (for the choledic duct and glandular acini of the liver are lined with a continuation of the same tissue on which the miasmata first act), and therefore secretion from this gland will be suspended and not increased.

At first sight, this view of the case will appear directly opposed to the law laid down by BICHAT, that irritation, being caused at the mouth of a duct, will be transmitted to the gland from whence such duct proceeds, and will give rise to secretion. But it yet remains to be proved, that the action produced by miasma is properly speaking irritation. The fact that chemistry has not yet detected its existence gives reason to believe, that the effects usually attributed to it are owing to the abstraction from the air of something which constitutes its wholesome stimulus rather than to the presence of an unwholesome principle capable of producing irritation.

Inasmuch as it is presumed that the action of miasmata on the nervous expansion of the mucous tissue tends to deprive that portion of the nervous system of its inherent power of discriminating the proper materials for secretion in the blood vessels; as moreover those materials are necessarily allowed to remain in the circulation; and as we know from analysis that the constituents of bile are very unfavourable to health;—may not their detention in the vena portarum be the cause of some of the anomalous phenomena of fever yet unaccounted for? The presence of bile in the blood has been denied; but its elements may certainly exist, and not be cognizable to our senses. The adoption of these views is not to be considered as implying a belief in the doctrines of humoral pathology. There is no primary disease of the fluids alleged, but a disordered condition only of those fluids, the consequence of a morbid action first developed in the solids. The following quotation from the writings of Dr SAMUEL JACKSON will support this view of the subject. "Is it not probable," says he, "that, in protracted diseases, attended with interruption of secretion and excretion, digestion and healthy nutrition, the quality of the blood may become depraved to a certain extent, so as to become incapable of a healthy restoration to irritability?"

The opinion is adopted by professor CHAPMAN, that miasmata, as a cause of disease, become entangled in the food and saliva, and thus enter the stomach. It would create but a slight modification

of disease, whether the primary impression were made on the stomach, or any of the arbitrary anatomical divisions of the intestines. The mucous tissue, which lines the whole, is most immediately exposed, and no doubt bears the brunt of the attack of the morbid agent. But it has been shown that miasma acting on this membrane cannot be supposed to cause an increased secretion of bile.

Some experiments have been made, intended to prove that miasmata are taken into the system by pulmonary and cuticular absorption. The authors of the most conclusive of these experiments tell us, that the proof of the presence of the deleterious matter in the system, was derived from its discovery in the blood and some of the excretions. Under these circumstances (allowing the theory to be correct), it must have affected the circulatory mass directly, in the manner already alluded to; and it has been contended, that we could not rationally expect increased secretion of bile during the existence of such a state of things. The general conclusion may be drawn, then, from what has been brought forward, that marsh effluvium, as a cause of fever, cannot, from any mode of its application, either directly or indirectly, influence the portal vessels to excessive elaboration of bile.

There are other causes said to produce fever, and among them solar heat is often mentioned. The ultimate effect of this agent on the system is atony of the extreme vessels, resulting from their previous great excitement, and, as a consequence of the former, the destruction of the balance of the circulation. In this state of things, it is the venous system alone which suffers, and it has been shown, that, when the veins are engorged, we cannot rationally expect undue secretion of bile. But there is also a negative proof that heat has not this effect; for if we attribute such a result to it, we virtually ascribe to it the influence of a universally pervading cause. In a country like that of Carolina or Georgia, where the heats of summer are oppressive, we should always be afflicted with those diseases (diarrhœa, dysentery, &c.) which result from redundancy of bile. But this is not the case. There are many places where diseases solely attributable to peculiarity of climate are not known. There are positions proverbially healthy; and so great is their celebrity, that they are thronged every summer with the ranks of wealth and fashion flying from the miasmatic districts of St Mary's, Darien, Savannah, Charleston, &c. The places alluded to are not

attended with any of the collateral advantages of mineral springs, &c. but are attractive merely on account of their salubrity.

The agency of heat in the production of increased biliary secretion may be looked upon almost as a settled doctrine in pathology. Amongst scientific men, the opinion owes much of its popularity to the support it derives from the celebrated JOHNSON, whose works are so much read in the United States. In the present essay, it has not been questioned, that heat may vitiate the bile, by causing an absorption of the serosities; but sufficient reason for denying the existence of that process in our febrile diseases has already been stated; namely, the absence of the first symptoms (diarrhœa, dysentery, &c.) that would most probably follow on that supposition.

Let us next inquire, whether solar heat has any peculiar power of generating a redundancy of bile. Dr JOHNSON's views are founded on a belief in the existence of a sympathy between the vessels of the surface and those of the liver, whereby an impression made upon the former will be propagated to the latter. Hence he supposed, that the application of heat to the surface, and consequent increase of perspiration, would be productive, sympathetically, of an increase of bile.

In studying the character of this theory, the following axioms in physiology should be kept in view: 1. The blood affords the pabulum for all the secretions and excretions. 2. There is but a definite quantity of this fluid in the animal economy; and from it, as a grand reservoir, all the secretory glands and depurating emunctories abstract or receive a part for their own proper purposes. 3. When, on the application of stimulus, any one of those glands or emunctories is excited to increased action, there must be a proportionate diminution of action in every other; but more especially in those connected with the excited organ by sympathy. The first of these axioms will not be disputed. If our second position be true, that there is but a definite quantity of the material of secretion, and if we suppose the already large portion of this material carried off by the skin to be augmented, may we not presume, as a necessary consequence, that every other gland and emunctory will secrete or carry off less? But if the third axiom be true, shall we not, for a still stronger reason, be entitled to infer, that increase of perspiration will cause decrease of bile? It is admitted on all hands, that increase of urine will cause decrease of perspiration, and vice versa;

but, according to Dr JOHNSON's theory, we are not to be content with supposing that the diminution of perspiration in diabetes mellitus is sufficient to balance the increase of urine, but must also suppose deficiency of bile in the same disease. Do reason and observation warrant us in believing that the quantity of urine secreted and carried off by the urinary apparatus is equal to the sum of the cutaneous depurations and hepatic secretions? A quotation from BICHAT may render the general position here assumed more tenable. "Heat," says he, "which expands the cutaneous system, increases its action at the expense of that of the glands, and reciprocally, cold, which contracts it, forces the glandular system to supply this action."

A very prominent objection, drawn from every day's observation, may be urged against Dr JOHNSON's theory. If it be an invariable law of our economy, that there shall be an increase of bile as a consequence of increase of perspiration, why is it that labourers, who are generally bathed in sweat, are free from those diseases which result from increase of bile? To account for the absence of diarrhœa in these cases, we are told by Dr JOHNSON, that nature establishes a vicarious sympathy in the mucous membrane, by which it is caused to secrete less mucus, and the intestines, being consequently destitute of their natural lubricity, do not so readily yield to the purgative influence of the bile. But if they do not yield to the *purgative*, will they not, when deprived of their mucous defence, more readily suffer under the *corrosive* influence of this "secretion when in a depraved condition?" Contrast Dr JOHNSON's opinion on the subject with that of Dr CLEGHORN. "Sharp, indigested humours, flowing from the stomach, or, which is more frequently the case, an increased secretion of bile and other liquids, necessary for chylification, produce a simple looseness, which gradually washes off the mucus from the intestines, erodes their membranes and at last brings on severe gripes with bloody stools." This paucity of mucus, which Dr JOHNSON thinks necessary to prevent diarrhœa, will, according to Dr CLEGHORN, cause dysentery in its worst form. But labourers are not particularly subject to either disease. Therefore the theory is improbable. Dr JOHNSON refers to some experiments of BICHAT, which prove that during the time of digestion in the stomach the biliary secretion is diminished; but that as soon as the food passes the pylorus it is increased. He traces the same analogy here, asserting that in the first part of the process the skin is dry and

hot, but when the food gets into the duodenum, the surface relaxes, and perspiration is much increased. Suppose the fact perfectly established, that the liver pours its bile into the duodenum immediately on the passage of the chyme through the pylorus, how are we to detect the time so precisely as to be able to say that there is an increase of perspiration at the same moment? We cannot tell from mere sensation. For since digestion is a process under the influence of the great ganglionic nerve, the sensorium does not take cognizance of it, and no one but the dyspeptic, whose stomach is in a pathological state, is ever sensible of the passage of food through the pylorus.

To enumerate all the causes of fever, and explain their modus operandi, would be but to repeat the main topics of the argument already presented. The uniformity of the symptoms of our fevers proves, that cold, and moisture, or the two combined, as well as acrid ingesta, all produce the same effects as miasmata and solar heat; the most prominent of which is the destruction of the balance of the circulation. And it has been proved that such a state cannot be reasonably supposed to produce a redundancy or depravity of the biliary secretion.

Inasmuch then as these states of the biliary secretion do not obtain, either as the cause or effect of fever, nor yet as the indirect result of any of the usual causes of fever, they may be assumed to have no connexion with febrile disease whatever. This supposed connexion between bile and fever has been copied by one physician from another, until its belief has been sanctified by age, and it is now deemed heterodox to depart from it, merely from the force of early impressions.

There are some signs which are usually considered diagnostic of a bilious habit, but which can be easily shown to have no connexion with it whatever. It is said, that in our fevers, we have dark, tar-like, or parti-coloured stools, which are known to consist of vitiated bile. In regard to these stools, let us inquire whether they are the necessary and natural attendants of fever? Are they ever seen previous to the exhibition of calomel? May not calomel by its energy (which we are told is principally expended on the liver) cause the flow of an unusual quantity of bile? But if it be asked whether calomel is capable of inducing so vitiated a condition of the bile, the answer is, that it probably can. This is a point which cannot be determined by experiment, and must therefore always remain problematical.

We may procure pure and healthy bile and genuine calomel; we may mix them and apply a temperature equal to that of the alimentary canal, but yet observation made on such a mixture would throw no light on the reciprocal action of bile and calomel in the system. We might add to the mass, the gastric and pancreatic juices, and suppose the peristaltic motion and the action of the different viscera to be severally unimportant to perfect the process, still there would be wanting an indefinite something, to render our deductions legitimate. Chemistry, with all the light it has thrown on modern science, cannot conduct us into the hidden recesses of the system, nor show us the modus operandi of the thousand agents concerned in the functions of the animal economy. It is certainly rational to suppose, that the vitiated appearances in the stools of febrile persons who have taken calomel may be the result of some unexplained combination of the fluids of the alimentary canal with bile and that medicine. Substitute any other medicine, and we shall still be in doubt whether a combination of the same kind may not take place.

A case however may be supposed, wherein the state of the excretions above alluded to may exist, independently of the exhibition of any purgative medicine. Dr Daniell, of Savannah, tells us that the evacuations which followed the application of sinapisms to the abdomen were very offensive. To account for this, without the necessity of supposing the bile to be primarily predominant or vitiated, it is necessary only to refer to the true state of the biliary secretion in our fevers. That it is diminished is proved by the constipation always present; and it is proved negatively by every argument in the former part of this essay. But better authority can be given. "Contrary," says professor Chapman, "to the prevalent notion, in the fevers emphatically termed bilious, there is a great deficiency of bile; sometimes, the liver being so torpid by the force of the remote cause, or by early congestion, that its secretions are suspended, and, until their restoration, we advance slowly with the cure." Now, on the application of a sinapism, the portal congestion is relieved, the circulation is equalized, and the blood, from which the elements of the bile are derived, being restored to comparative health, the secretion will return in increased quantity, proportioned to the time of suspension. From detention in an unnatural situation, it is probable also that it will have acquired a vitiated appearance and offensive character.

Yellowness of the eyes is looked upon as one of the signs of a bilious habit. The colour is often said to exist, when it cannot be perceived, unless, forsooth, the physician takes the trouble to press down the lower eyelid, when a yellow tinge can be seen, which in a great majority of instances is apparent in perfect health. Dr FORDYCE suggests, that this is owing to a redundancy of the sebaceous matter, natural to the part. Dr DANIELL, on the contrary, thinks it the result of a diseased state of the capillaries. But let us admit that it is the result of bile; then so far from this appearance indicating that the biliary secretion is excessive, it merely indicates that the usual quantity is obstructed in its passage into the duodenum by some pathological state of the ductus communis, or the gall-duct proper. It is the cystic bile (if it be bile at all) which is diffused over the system, whether it be by absorption or regurgitation. But the manner in which the appearance is explained by Dr DANIELL is infinitely more satisfactory.

The fact has been much relied on, that, as a precursor of our common autumnal fevers, we have a vomiting of bilious matter. This does not prove, however, that there is any derangement deeply implicating the biliary secretion. Among those causes that produce fever, there are some whose primary effect is to cause nausea and vomiting. This nauseating cause may be so extended as to invert the peristaltic motion in the duodenum; hence the choledic duct may be emulged, and healthy bile, being brought into the stomach, will in most cases be ejected. If it be more decided in colour, or bitter in taste, than natural hepatic bile, we might readily explain these qualities, by supposing the gall bladder (which, owing to the contraction of the abdominal muscles in vomiting, is pressed upon by the surrounding viscera) to have evacuated its contents. If we were called upon to give a correct name to the fluid thus vomited, it would be wrong to call it bilious matter, even though we had reference to natural bile only. Dr FORDYCE suggests, on this subject, that we are sensible of the presence of bile, because it is rendered conspicuous by its characteristic properties of bitter taste and smell, and decided colour; while the gastric and pancreatic fluids (being no doubt also present) are deficient in similar striking properties. The choledic duct opens into the duodenum, and were bile superabundant or acrid, it is more probable that it would produce its natural effect (diarrhœa or dysentery), than that it should by sympathy nauseate and cause vomiting. One result we can explain, by referring it to actual contact and

direct action; the other can only be accounted for through the medium of the more mysterious agency of sympathy.

It may be objected to this paper, that even if the position assumed in it were true, it answers no practical end; or in other words, that though redundancy or depravity of bile have no connexion with fever, yet deficiency of the same fluid may have great agency in the disease. Accordingly it may be said that the term bilious and the mercurial practice will be equally proper on both suppositions. The term however is objectionable, because it will uniformly convey the idea of superabundance of bile. In regard to the unity of the practice in deficiency, redundancy, or depravity of the bile, the enlightened physician need scarcely be reminded, that, although the same medicine may be used in all these cases, yet the mode of administration in each is quite different, and followed by widely opposite results. In the case of redundancy of bile, supposing it to be too bland to carry itself off, the maximum dose of calomel is given, followed by laxative medicine; thus extending its influence so far only as to cleanse the primæ viæ and carry off the offending matter: but in deficiency of bile, arising from torpor or congestion of the liver, the calomel is given in small and repeated doses, in order to secure a stimulant operation on the liver, by causing its effects to accumulate in the system in a gentle and gradual manner. The secretions are thus solicited and not forced; and the patient convalesces without that afflicting and permanent derangement of the digestive functions so often the consequence of the exhibition of large doses of purgative medicine. That this derangement is often the sequel of a mercurial treatment, we are the more easily persuaded to believe, when we reflect on the cruel and unnecessary waste of the gastric, pancreatic and biliary fluids which it occasions,—the high grade of excitement to which the organs which furnish those fluids are necessarily subjected,—and the subsequent debility and collapse which they must suffer. This practice with mercury especially demands reform in South Carolina and Georgia, where calomel is so exclusively relied on in fever.

ARTICLE IV.—*Observations on the Injurious Effects of the Stimulating Treatment applied to Articular Tumours which appear to have passed to the Cold or Chronic State, but which nevertheless require a continuation of Antiphlogistics—or a soothing method. Addressed to the Kappa Lambda Society of Philadelphia. By* M. CHAUFFARD, *Principal Physician of the Hospitals at Avignon, Vaucluse. Translated by* D. THEODORE COXE, M.D.

[We are indebted to Dr LA ROCHE, one of the Editors of this Journal, who is now in Europe, for forwarding the two following very interesting papers. As many of our readers do not read the French language, we have deemed it best to publish a translation of them, and we are indebted for the English dress to Dr D. THEODORE COXE.]

It is generally admitted that inflammatory diseases long treated by emollients finish by requiring the use of tonics; the more plausibly in proportion as the tissues are less susceptible, less porous, less permeable, less accessible to the sanguineous engorgement. Thus, in most works and most consultations, it is recommended to treat by excitants, after a longer or shorter period, according to the individual, all those tumours which are developed around the articulations, and which seem to have their especial seat in the periosteum, the aponeurotic extremities of the muscles, the ligaments, the fibro-cartilages, and the dense cellular tissue interposed between all these.

This method, which is willingly extended to all cases, sometimes however fails of success: and its obstinate application keeps up, on the contrary, in certain circumstances, the morbid accidents; increasing them, and not always being innocent of the irremediable alterations of texture which bring with them the loss of limb or life.

On this subject I offer some observations.

In 1816 M. C. 20 years of age, made a false step, and immediately there ensued a considerable engorgement of the whole left tibio-astragalian joint. Very proper means were immediately put in practice; first refrigerants, then cataplasms and local bleedings. The inflammatory fluxion was of the most intense kind, and a month after the accident there remained much swelling and pain upon the slightest motion: nevertheless, the skin was pale and rather shining,

as though erysipelatous, being depressed with ease, and retaining for a long time the impression of the finger. The individual was habitually pale, of a phlegmatic temperament. Tonics were judged necessary, and the moment of administering them appeared to have arrived. Under their influence, the tumour enlarged, dull pains were more constantly felt, the patient became paler, the complexion wan, and the organic functions languishing. The swelling extended to the whole lower extremity, and the health declined—a stronger reason for insisting upon the treatment which seems so appropriate to such a state of general and local weakness. M. C. departed for the waters of Grevulx, but found no amelioration there; on his return he wished for no more remedies and confined himself to keeping the parts warm and in the most absolute repose. A cure took place, but very gradually. This patient could not walk for a year, and then used crutches for more than six months.

False ideas, resulting from the aspect of the skin, the continuance of the swelling, the existence of a lymphatic temperament, make us forget that the phlegmasiæ of the articulations, proceeding from a violent shock, are seldom dissipated, but with extreme slowness; thence is derived a stimulating system of treatment, entirely opposed to the nature of the disease. The sad consequences are referred to the weakness of the individual, possessed as we are with the idea, that after a given period excitants are demanded. In addition, it is not for a moment suspected, that the languor of the functions may, with the other symptoms, depend, at least in part, upon the tonic treatment so unhappily substituted for the emollient. Without the abrupt determination of our patient, who can say he would not have died, or that the softening of the whole articulation would not have necessitated the amputation of the limb?

Mademoiselle Gasp. 14 years old, having menstruated for some months, her neck studded with suppurating scrophulous ulcers, was attacked in 1819, with a considerable engorgement of the left knee. She had a very white skin, considerable fat, the upper extremities pretty large, and the lower extremities very small. The engorgement of the knee was developed like a white swelling; it became hard, little sensible, and so large that the leg could not be bent upon the thigh. Internally, she took the martial preparations, wine, strong meats, the cruciferæ, sulphur, bark, syrup of Portal, elixir of Peyrylhe: externally, were applied fumigations, aromatic bags, cam-

phorated brandy, stimulating frictions, alkaline baths; for a month douches, at first alkalized, afterwards sulphurous. Under the influence of all these means, which appeared however so strongly indicated, there was no amelioration, but extreme emaciation of the limb, sensible increase of the tumour, manifest decay, the ulcers of the neck deep and spreading, with ill-conditioned pus. We at last changed this treatment, thinking we could perceive an obscure fluctuating movement upon the lateral posterior parts of the tumour. A large emollient cataplasm was applied entirely round the knee, and she took white meats, eggs, panada, turtle and snail soup, milk, and white decoction. These mild measures being perseveringly continued, the health was gradually restored, the ulcers of the neck cicatrized, and the whole mass of cellular lardaceous tissue, engorged with lymph or serous fluid, which constituted the tumour of the knee, was converted into pus; fluctuation being sufficiently evident, an opening by caustic was made in the sac, and issue given to a quantity of pus of a pretty good quality. The openings gradually healed, and the cure, though very slow, was complete, constantly under the influence of the same regimen, and of local emollients. The girl now enjoys excellent health, uses her leg freely, and walks as if the limb had never been diseased.

The conclusions to be drawn from this case are so evident, that it is almost useless to enumerate them. In short, no morbid state seemed better adapted for stimulants, and nevertheless, they were constantly useless, and kept up a sort of hectic fever, contributing to the emaciation, probably through the medium of a chronic gastro-enteritis, of which they were the source: they operated disadvantageously upon the progress of the ulcers, and the scrophulous tumour. Very different results and a cure followed purely analeptic and emollient measures.

In 1821, Bl. 12 years old, was brought to me from a neighbouring village, pale, thin and suffering from a considerable tumour around the external malleolus of the left leg, and a pretty large wound upon the external lateral face of the foot, at the inferior part of this tumour. These were all consequent to a fall the child had had from a cart, in which this foot was much bruised. At the commencement of the disease, emollients were used; but as the cure was not as prompt as was expected, recourse was had to topical excitants, with a view of resolving this engorgement. From this time the disease increased, and œdema gradually seized upon the whole

leg. An ulcer was then formed, and, in spite of ointments, continually spread, furnished a serous pus, and remained tender and wan. I advised simple dressing with pledgets of lint, surrounding it with a fine linen bandage smeared with cerate, and covering the whole joint with a poultice of flaxseed, or bread-crumbs boiled in a decoction of marshmallows; to keep the limb also perfectly quiet, to give the child goat's milk three times a day, with barley water sweetened; and to nourish him with fecula, eggs, and other analogous food of easy digestion. Four months after commencing this treatment, the health was restored, the wound healed, and the tumour almost entirely gone.

Had the *pretended resolvents* been continued, this engorgement would certainly have degenerated into a white swelling, perhaps incurable. The best books, too, lean to this erroneous determination. "The use of these topicals [emollients]," says M. Boyer, "is continued as long as the pain and inflammatory engorgement exist; but as soon as they disappear, recourse should be had to resolvents, such as aromatic wine, camphorated brandy, &c.*" Precepts too absolutely expressed, since in similar circumstances to the one in question, *though there may be neither pain nor inflammatory engorgement*, there still remains an organic susceptibility which these measures heighten sufficiently to revive serious accidents, to perpetuate a dull phlegmasia, &c. Besides, since the most absolute rest is recommended, even long after the success of the treatment, *for fear of keeping up irritation and inflammation*, p. 13, why advise measures which so often lead to this result? It appears to me a contradiction.

Subsequently to an acute articular rheumatism with which Mademoiselle Cré was affected, continual pains remained, situated principally over the right instep. Thence there resulted a gradual displacement of the os scaphoides which exposed the head of the astragalus, and at the same time a well marked incurvation of the foot outwards; in addition, there were pain and habitual engorgement of this extremity, which exercise and even standing increased. In this state, in April 1825, the patient, whom I had not yet attended, went to Montpelier, to consult M. Delpech. She was 17 years old, menses regular, in good health although of a lymphatic temperament; and yet, notwithstanding these propitious circumstances, this

* Mal. Chirurg. Tom. IV. p. 12.

able practitioner was fearful lest a white swelling should be formed in the midst of the spongy bones of the tarsus. He consequently ordered strict rest in bed, temperate regimen, light food, such as white meats, vegetables and river fish; saponaceous injections, purgative and tonic pills daily, as long as they did not irritate the bowels, and produce more than one or two stools in the 24 hours. Every eight days, leeches in the neighbourhood of the deformity; twice a day, for an hour, a local bath of a decoction of the leaves of solanum dulcamara, followed by an embrocation of oil of hyoscyamus, and then a poultice of rye meal and flaxseed; subsequently and upon the visible decline of the disease, caustic potash was applied between the tibia and tendo Achillis, two inches above the line of the internal malleolus: the treatment concluded with the use of the waters of Mont d'Or.

The chief aim in these directions was, a temperate regimen, complete rest of the part, frequent local bleedings and topical emollients. It seemed however so natural, considering the date of the disease, the nature of its cause, the presumable degeneration of an acute into a chronic rheumatism, and the *atonic* swelling of these soft bones, to prescribe dissolvents, resolvents, topical excitants and douches of strong mineral waters, natural or artificial! Nothing was joined to this simple and luminous plan but revulsives to the intestines, and they scarcely influenced the cure; for the patient being incommoded by them, I suspended them several times according to the spirit of the directions which had been confided to me. As to the cautery, she was as it were well when it was used, and M. Delpech, who again saw her at this time, rightly judging it useless, discontinued it. The cure was already so complete, that the journey to the waters of Mont d'Or caused neither displacement nor swelling, and was merely followed by œdema of the subcutaneous cellular tissue, which vanished after two days' rest.

Mademoiselle Cré now stands nearly all day behind a counter, and has even laid aside her boots which were lined on the inside with a plate of steel to sustain a part once so much deranged.

In the summer of 1825, young Matilda B., returning with her father from Bourg to Lyons, rested all night on her left wrist, her hand being shut. Torpor and pain in this joint were the result; and eight or ten days after, the motions of this hand became difficult, and she could no longer play on the piano. Compresses dipped in brandy were applied round the joint, and the patient conti-

nued to use the hand as well as she could, making great exertions. The wrist gradually swelled, and at length all motion was lost, the hand seeming to be lengthened and always hanging. Her parents then quitted the country and went to Lyons. The most careful attention was paid her by Mess. Bouchet and Baumers, and an antiphlogistic and emollient local treatment adopted. The patient, though thin, and slightly developed for her age, enjoyed excellent health. She persevered for some time in the same plan, but no amendment took place. The wrist being much deformed and swelled, with no redness of the skin, and very little pain, these gentlemen supposed the really inflammatory period of the affection had passed, and tried slightly stimulating douches, the application of aromatic bags, and then a pretty tight bandage of flannel, surrounded by oiled silk*. Instead of diminishing under these new measures, the tumour still increased, without however showing any external sign of cutaneous phlegmasia. Matilda was sent to pass the winter in the south, that the functions of the skin being there favoured by the influence of the sun, her disease should not at least be aggravated, if this extensive revulsion should continue inefficacious. I was entrusted with the care of the patient, and for some days continued the above mentioned bandage, carefully examining the tumour. Whether a more decidedly inflammatory action now began, or whether the fatigue of the journey excited its development, I soon perceived the necessity of returning to the original treatment. Leeches on several parts of the tumour, baths and emollient cataplasms were applied, and a slight diminution of the engorgement resulted; but the skin reddened at the palmar surface of the joint, pus collected and finally escaped by a spontaneous opening; it was serous and ill conditioned. This opening enlarged, and a ragged ulcer succeeded. The same treatment was continued, and a new collection of pus was formed at the right side of the joint, and afterwards on the left: this necessitated the application of caustic potash. The suppuration of these wounds progressively amended, and at the end of two months, the hand was less pendent, the tumour much smaller, and the wrist not so deformed. The resolution of the tumour however was far from being complete, and the ulcers did not heal. I then tried, still continuing the same

* The arm had been, from the commencement of the treatment, in a sling; the hand supported by a splint, and consequently immoveable.

local treatment, to give more activity to the absorbents of the limb by friction, with half a drachm of strong mercurial ointment, upon the arm and forearm. No effect being appreciable, I communicated the facts to my learned brethren of Lyons. Their opinion was, that the active movement of the tumour must have ceased, that the articular engorgement was taking on the chronic character, and consequently, that the relaxing treatment had better be laid aside, and local baths applied, composed of a solution of Flemish glue and hydro-sulphuretted liquor. Light douches upon the joint, of the same mixture, at a suitable temperature; bags with a mixture of powdered henbane, chamomile, elder and carbonate of magnesia; frictions upon the engorged parts with hydriodate of potash ointment; and a flannel compress bandage, were also recommended. In the meanwhile, professor LALLEMAND, who was passing through Avignon, saw the patient, and gave a similar opinion. He thought that local alkaline baths, general baths, with a decoction of sage, lavender, rosemary, and an addition of two glasses of brandy should be used; and afterwards sea bathing, &c. I acquiesced in this unanimity of sentiment, proceeding from such enlightened men, and thenceforward used baths slightly alkalized and general stimulating baths alternately, with the bandage, intending subsequently to use the hydro-sulphuretted douches, the aromatic bags, &c. Ten days had not passed away when the tumour, instead of diminishing, seemed to increase, pains were felt, and the little ulcers began to bleed. We had finally to return to the same emollient measures, which calmed this momentary excitement, brought things back to their former state, and at length completely cured the patient. When she returned to Lyons, the ulcers were cicatrized, the wrist was very little deformed, and the hand could be moved pretty freely. I now merely applied a roll bandage, and convalescence continuing, the mother living in the country, nothing more was done.

In this case again certainly, the cessation of the emollients, and the application of tonic means seemed indicated in every way. When however they were employed, bad consequences soon followed.

I do not report these facts with the intention of absolutely proscribing, from the treatment of these articular affections, the use of tonics (I am not ignorant that cases exist where they are followed by success); but solely to show, that at a period, improperly determined by theoretic ideas, there is danger in prescribing them indis-

criminately, and that in many circumstances a milder method can alone accomplish a cure.

Of the five cases I have reported, none is furnished by an adult; and the most obstinate disease is that of the eldest of the individuals mentioned, which confirms, in my opinion, this axiom of M. Boyer:

"In general, and under ordinary circumstances, white swellings are more serious in feeble and cachectic subjects than in the well made and healthy, and in young people and adults than in children." *Op. cit. p.* 507.

ARTICLE V.—*Removal of the Penis, by means of Dr* MAYOR'S *Constrictor, or Ligature Tourniquet. By* M. FRANCOIS A. BINET, *Surgeon of the Hospital De la Croix at Nice, Piedmont. Communicated for this Journal. Translated from the M.S. by* D. THEODORE COXE, M.D.

M. B. seventy-two years old, of a strong constitution, bilious and sanguine temperament, having never been seriously ill, had entered very young into the navy. He was married at thirty, and in a few years had four children, one of whom is still living. At the end of twenty-three years of wedded life, his wife died of disease of the breast, and after three years of widowhood he again married; his second wife is still living, enjoys excellent health, and has made him the father of five children, all equally robust and healthy.

During the life of his first wife M. B. after a journey perceived that his prepuce became gradually contracted; as he did nothing for it, in six months he had a phymosis. This state, the cause of which M. B. never could imagine, was present when he remarried.

Some years after, the emission of urine having become very difficult from the almost cartilaginous induration of the opening of the prepuce, an inconvenient itching between this and the glans was felt, which increased so much, that M. B. was almost constantly forced, even during sleep, to scratch it violently with his hands, which produced a secretion, and flow of very fetid lymphatic fluid.

M. B. now consulted a surgeon, who thought he saw in this fetid secretion, and in the phymosis, an old syphilis, and treated him accordingly*.

A vast number of mercurial pills, frictions of mercury along the penis, VANSWIETEN's liquor, and many other remedies were employed, but without any amelioration; on the contrary, the penis increased in size and became painful. Two respectable surgeons of this place, being consulted, unknown to each other, disapproved of the medical treatment M. B. had undergone, and advised baths, emollients, and a treatment capable of arresting the evil and preventing the cancerous degeneration of the penis which might have been the consequence.

M. B. paid no attention to this wise advice, but preferred using the universal remedy, LE ROY's purgative, and took about nine bottles of the different numbers of this drastic in the space of five months; he then passed to the use of many internal and topical remedies, which quacks extolled as infallible, but all in vain: the penis became more and more painful, and of such a size, that the circumference of its extremity was nearly seven inches; the skin which covered this part was stretched, shining and livid; and the secretion from the prepuce was greatly augmented.

The weight of the penis and the pain prodigiously increased, and fever and emaciation soon reduced him so low that he could no longer quit his bed. He was in this miserable state when another quack undertook him.

Topicals of every description were employed by this last, but in less than a month several holes appeared in the prepuce, which gradually extended along the penis even to its root. Fungous excrescences which were infinitely multiplied, appeared across these holes, and in a few months the penis was entirely covered by them.

M. B., alarmed at the progress of the disease, called me in, (May 1827) and I found him in the following state: extreme paleness; great emaciation; a frequent, small, feeble, extremely irregular, and often even intermittent pulse: insupportable pain day and night in the penis, the end of which was as large as the fist; prepuce and

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* M. B. constantly assured me, that neither he, nor his two wives had ever been affected with any syphilitic disease.

glans so changed as not to be recognised; the whole penis was a mere mass of fungous excrescences of a cancerous nature, like a decayed cauliflower, studded with livid points, and hollowed out by deep fissures, whence the urine flowed as from a watering-pot; an abundant ichorous and very fœtid suppuration assailed the nose at a distance. Round the root of the penis, in a radius of ten or twelve lines, and an inch and a half towards the scrotum, the skin was livid and hard, and full of little varicose blood-vessels, and callosities or tubercles of the size, nearly, of hemp-seed. The right inguinal glands were very little engorged—the left on the contrary very much so; two of them were as large as common nuts. The patient had kept his bed for about ten months.

My first care was to endeavour to moderate the pains, which gave him no respite; and to restore his strength by food of easy digestion: the penis was dressed with lint, dipped in a tepid solution of sedum minus and carrots, to which were added, at each dressing, a few drops of cherry laurel water. In twenty days the patient gained strength; the pains were mitigated; the swelling of the inguinal glands nearly subsided; and the fungosities of the penis so far diminished, that for one while I thought they would entirely disappear. These appearances however being but deceptive, and the disease continuing to progress towards the root of the penis, I concluded that, to save the life of the patient, recourse must be had to amputation. This operation was more promising, inasmuch as the disease of M. B. did not present the characters of a cancerous diathesis; neither did it depend upon a syphilitic affection, but solely on a very old, accidental phymosis*.

I then desired two distinguished surgeons, old friends of M. B. to visit him with me. They agreed with me as to the propriety of the operation, and that the moment was still favourable for attempting it. We visited him a second time two days after, to examine more carefully the state of the urethra, and the root of the penis. The urethra, examined in the perineal region, presented no callosity, irregularity, nor augmented sensibility.

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* The authors Roux and William Key report a great number of cases of amputation of the penis for cancerous affections. Most of those operated on, who were radically cured, were advanced in life, had not been affected with syphilis, and the development of the cancerous affection had been preceded by a phymosis dependent on some constitutional or accidental but always old defect.

Among the fistulous erosions, whence, as I have said, the urine flowed, we observed two, placed laterally at the root of the penis, much larger than the others; into these I had before introduced and kept up for two days a pretty thick dossil of lint, the better to ascertain their direction and depth. Examined by the blunt probe, we found they were lined throughout with the same fungous excrescences as the penis, and penetrated the lower part of the symphysis pubis to where the urethra passes over the bifurcation of the corpus cavernosum. Should we then amputate the penis in the ordinary way, or was it better to try Dr MAYOR's mode *?

I was strongly inclined to the latter, as it was not at all prudent to cut off the penis far under the symphysis pubis; the operation would have been tedious, painful and very difficult; and the hemorrhage, in spite of every possible care to stop it, might have had bad consequences, particularly in a patient weakened by age and suffering, and whose pulse was small, irregular, and often intermittent. I therefore determined to procure the ligature tourniquet, and in the meanwhile, the patient continued the treatment adopted, with the addition only of prepared sponge, by means of which I succeeded in a few days in considerably enlarging the two lateral fistulæ at the root of the penis, and in more fully ascertaining the direct communication which existed between them and the urethra, beyond the bifurcation of the corpus cavernosum. The 24th of September last, at eight A. M., provided with a constrictor, and assisted by two very intelligent young surgeons, I operated in the following manner.

I cut the skin around the root of the penis, where it ceased to be livid, hard and tuberculous; I then hastened to isolate this member beyond the root, by dividing with the fingers the cellular tissue which here surrounded it, and with the bistoury some aponeurotic fibres, and its suspensory ligament. Four small arteries were cut in this operation, but as I hastened to tie them, the hemorrhage was trifling. I then placed the constrictor as deeply as possible, i. e. upon the bifurcation of the corpus cavernosum, near where the urethra was opened by the lateral fistulæ of the root of the penis†, which facilitated my

* Dr MAYOR's method, the success of which has astonished many operators, is intended to remove by ligature certain tumours and degenerated organs, I am not aware, however, that any other has up to the present time employed it for the removal of the penis.

† I did not think it necessary to attempt passing, with the constrictor, the part where these fistulæ terminated in the urethra, which would have been painful

drawing the penis in its horizontal direction; I then made a transverse incision upon the inferior surface of this last, about four lines on this side of the constrictor, to discover the urethra, into which I introduced a very short silver catheter, which I pushed about an inch beyond the constrictor: this I made gradually act, until the patient experienced a supportable sense of constriction, proposing to tighten it the next day.

The operation being thus finished, I covered the whole with a simple dressing. For half an hour M. B. complained of a pretty sharp pain, without being able to assure himself it was the effect of the constriction of the penis; but he afterwards conversed very cooly and tranquilly, complaining of no pain: the urine flowed through the catheter. About 2 o'clock in the afternoon, he had a severe chill which lasted until four, when warmth returned. At five I found him moist, with no pain in the part operated on; the fungosities of the penis discoloured and faded; and the pulse still small, very frequent, and soft without intermission.

I directed one of the young surgeons, who was to be with the patient during the night, to relax the constrictor, in case he perceived its action likely to cause spasms or very acute pain. I told him, it was the more necessary to be careful in this respect, as the constrictor being deeply placed and on an erectile organ provided with many nerves, and endowed with great sensibility, it might cause serious morbid phenomena.

The 25th, I learnt in the morning that the patient had been agitated by a burning heat till midnight, and did not sleep till towards 3 o'clock, after having soaked four shirts with perspiration. I was at the same time agreeably surprized at the report from the young surgeon respecting the constrictor, which he had just removed, observing, that the penis was in a state of complete mortification. A few hours afterwards, I called to see M. B. He was very calm, complaining of no pain; his pulse was less frequent than the evening before, but still small, and without intermission. The penis exhaled an intolerable stench, and was putrid. I took away with the scissors all on this side the opening by which the canula penetrated the urethra; lotions of tepid water were applied this and

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and difficult; besides I was persuaded the action of the constrictor would cause mortification in a line or two of the penis beyond the point upon which it acted: I also foresaw that the internal sides of the above mentioned fistulæ could not escape the action of the constrictor.

the four following days, at the end of which time, the rest of the penis, which was mortified by the constrictor, separated entirely*. The wound then assumed a very fine colour, and was dressed with cerate. The metallic catheter was changed for a gum elastic one.

As, when I operated, I did not spare the skin, and was obliged to cut it to within an inch around the root of the penis, and even more on the side of the scrotum, a considerable space of an ovoid figure was left between the lips of the wound, whose larger end was towards the linea alba, and the smaller towards the scrotum; this space was about three inches in the centre, and five and a half from one end to the other. This however I did not mind, as M. B. having, in an advanced age, passed from a state of embonpoint to one of emaciation, the skin was pretty ample; it could readily slide upon the subjacent layers, which permitted the approximation of the edges of the wound, by means of simple adhesive strips which I used with the greatest possible success.

M. B. quitted his bed on the 20th of October. On the 2d of November he began to acquire strength and flesh. The wound is now closed; the skin is cicatrized in the form of a funnel an inch in diameter, at the bottom of which the opening of the urethra is perceived, which is about an inch and a half below the surface of the skin. He urinates with much ease, the water being often propelled nearly a foot; and the scrotum has become entirely pendent and of a round figure.

Reflections.—Writers upon the operations of surgery make no mention of the amputation of the penis, at the part where I applied the constrictor, in M. B.'s case; the principal motive for which silence I conceive to be, that in a similar case, after the operation, the divided arterial vessels would be very deeply hidden, and conse-

* The removal of the penis by the constrictor did not cause acute suffering to M. B., as he experienced after the operation, and that only for half an hour, a vague sensation of pain, which he was not even sure was occasioned by the constrictor. As further proof of this, the morning after the operation M. B. asked me, if it would be long before I amputated the yard, an operation he dreaded on account of the pain; when told it had already been done since the last evening, he wished to be sure of it himself, unable to believe that the excision of a part so sensible as the penis had caused him so little pain.

quently too difficult to be seized upon for arresting the effusion of blood.

Dr MAYOR's method being the only one capable of obviating so great an inconvenience, the profession doubtless owes him great acknowledgments; and I must certainly say, to his praise, that if I had had no knowledge of his constrictor, I should never have attempted the operation upon M. B., and he would inevitably have perished by the progress of his disease, and amidst great sufferings.

ARTICLE VI.—*History of a Remarkable Case of Obstruction of the Bowels which ended in Recovery. By* FRANKLIN BACHE, M.D. *Physician to the Philadelphia Prison. Read before the Kappa Lambda Society of Philadelphia, 23 April* 1828.

Matilda Bayard, a stout, short mulatto, aged 15 years, was admitted into the hospital of the Philadelphia prison on the 27th of January 1827. She complained of pain over the abdomen, and her bowels had not been moved for several days. Ordered five grains of calomel, to be repeated every four hours until the bowels are moved.

Jan. 29th. Bowels not having yielded, and dysuria being present, —omitted the calomel, and ordered a mixture of sweet spirit of nitre and sulphate of magnesia, to be assisted by injections of the latter salt. The pain over the bowels having increased, ordered twenty cups to the abdomen.

31st. Bowels not moved. Directed the warm bath, and the mixture of sulphate of magnesia and sweet spirit of nitre, and the injections to be continued.

Feb. 1st. No alvine evacuation yet obtained. Omitted the above remedies, and ordered half a grain of the extract of elaterium to be given every hour, until the bowels should yield, to be assisted by injections, containing spirit of turpentine.

2d. No stool. Omit elaterium, and resume the mixture of sulphate of magnesia and sweet spirit of nitre, and saline injections.

Feb. 7th. Between 2d and 7th, the sulphate of magnesia mixture and injections were the principal remedies employed, and the bowels were slightly opened. This day, the patient appearing to be worn out with pain and much exhausted, ordered calomel gr. iii. with opium gr. ½, three times a day, and a nourishing diet, consisting of beef tea, chicken broth, and arrow root and milk.

9th. Bowels not open, and dysuria continues. Omitted the calomel, and ordered the compound tincture of aloes, and uva ursi.

12th. Since the 9th, has taken the compound tincture of aloes, and uva ursi. The bowels, however, have not been properly opened. This day my friend, Dr Thomas Harris, saw the patient with me, and recommended a combination of calomel and opium. Accordingly, ten grains of calomel and one grain of opium were directed to be taken three times a day. At the same time, thirty drops of the spirit of turpentine were ordered, to be repeated at the same intervals, with a view to stimulate the kidneys and bowels.

13th. No stool having been obtained, the calomel was discontinued, and four half-grain pills of elaterium ordered, to be given at intervals of two hours.

14th. The bowels still refuse to yield. Ordered twenty cups to the abdomen, fomentations, sulphate of magnesia, and injections.

16th. No motion yet obtained. Ordered twenty cups to the small of the back, half-grain doses of elaterium every two hours, and the fomentations to be continued.

17th. Discontinued elaterium: ordered cream of tartar and jalap, which was continued on the 18th.

19th. Since the 17th, the bowels have been opened, but not completely. I now determined to bring the patient fully under the influence of mercury, if possible. Accordingly five grains of calomel and one grain of opium were directed three times a day, and frictions with mercurial ointment.

21st. The bowels becoming again closed, ordered cream of tartar and jalap every other day, and the calomel and opium to be continued on the intermediate days.

23d. Ordered injections, to be repeated daily. The pudendum having become inflamed and swelled, directed a poultice of bread and milk.

March 1st. Since the 23d ult. the cream of tartar and jalap, and the calomel and opium have been continued on alternate days, as well as the injections, but without producing a stool or affecting

the mouth. Omitted these remedies, and resorted again to the elaterium in half-grain doses during this day. The poultice was discontinued.

March 5th. The patient being worn out with pain and fatigue, and despairing of getting her bowels opened, ordered two grains of opium at bed-time.

9th. Complains of soreness of the mouth. Ordered two ounces of sulphate of magnesia, and the opium to be diminished to a grain and a half.

12th. The bowels have not been moved, and all the symptoms becoming aggravated, such as violent pain over the bowels, with distention of the abdomen,—ordered twenty cups to the abdomen, a powder of cream of tartar and jalap every two hours during the day, and the opium to be continued at bed-time.

13th. Ordered the warm bath.

14th. Sulphate of magnesia mixture and injections were tried, but without success. On the 16th, the elaterium was resorted to again.

17th. Twenty cups were directed to the abdomen, and a blister to be applied immediately afterwards.

19th. The elaterium was discontinued, and the sulphate of magnesia mixture resumed.

21st. The elaterium was resumed, three half-grain pills being given.

22d. Much pain existing in the loins, and there being an almost constant suppression or retention of urine, I this day formed two caustic issues in the small of the back, in the hope that they might produce a salutary impression on the kidneys and bowels by counter-irritation, or the discharge they might create. The opium at night was increased to four grains.

24th. The bowels have yielded to a certain extent, and the patient appearing to be weak, ordered a pint of wine and half a pound of mutton daily.

25th. The patient appearing to be sinking, I ordered the volatile alkali, beef tea, and half a pint of brandy, instead of the wine. Reduced the opium to three grains at night.

April 1st. Since the 25th ult. the remedies have been elaterium, (on the 27th), sulphate of magnesia, injections, and opium every night. This day, the symptoms becoming again formidable from the obstruction, and the danger appearing more imminent from the state of the bowels than from the existing debility, ordered the ela-

terium once more, to be assisted by fomentations and sulphate of magnesia. Continued opium at night.

April 3d. Ordered thirty dry cups to the abdomen. Continued the sulphate of magnesia, and directed injections.

4th. Discontinued the brandy, and ordered half a grain of elaterium every two hours. Continue opium at night.

In the foregoing account of this case, the general character of the symptoms has been recorded from memory, or inferred from the remedies employed, of which a register was kept from day to day. From this time, however, memoranda of the principal symptoms were made at the bed-side.

5th. This day, had a violent vomiting, and threw up fæces. Ordered injections of sulphate of magnesia, to be repeated every two hours.

6th. Feels no pain in the abdomen, unless when considerable pressure is made. Directed powders of cream of tartar and jalap, and the enemata to be continued.

7th. No evacuation per anum having taken place, the cream of tartar and jalap were omitted, and twenty grains of calomel ordered, with a continuation of the enemata. The stomach being sick, directed the effervescing powders.

8th. No evacuation. Repeated the calomel, and continued the effervescing powders, and enemata.

9th. Vomited fæces. Since the 4th, three grains of opium were taken pretty regularly at bed-time.

10th. Resumed the use of the cream of tartar and jalap, and discontinued the enemata, the patient being fatigued and rendered sore by them.

11th. Examined the caustic issues, and finding them disposed to heal, discontinued the poultice, and ordered the savine ointment. The bowels continuing still obstructed, and the abdomen being distended and painful,—ordered twenty dry cups over that part, and the elaterium to be resumed, to be assisted by cream of tartar and jalap.

13th. Yesterday, took four half-grain pills of elaterium, and two powders of cream of tartar and jalap, without producing a motion. Ordered an injection, containing ten grains of tartarized antimony.

14th. The injection of tartar emetic produced a copious stool yesterday; and the abdomen, which before was very much distended, became considerably less immediately afterwards. Notwithstand-

ing this relief, the patient vomited fæces to-day. Stop the cream of tartar and jalap, continue elaterium, and repeat the injection of tartar emetic.

April 16th. No stool since the 13th. Continue elaterium and resume the injections of sulphate of magnesia. Foment the abdomen.

17th. The symptoms derived from the obstruction being all aggravated, I discontinued the elaterium and opium at night, and ordered twenty-four cups to the abdomen, the warm bath, two ounces of metallic mercury, and injections of warm water.

18th. The injections yesterday succeeded. Eleven syringefuls of warm water, amounting to about two quarts, were thrown up, and immediately produced a large stool, containing three or four scybala, about the size of an unshelled walnut. After an interval of a quarter of an hour, eleven syringefuls more were injected, and caused a smaller stool, containing two scybala, about the size of a hickory nut. The injection being repeated a third time, was followed by a third stool, consisting of liquid fæces.

19th. The bowels having yielded, I determined once more to attempt to bring the patient under the influence of mercury. Accordingly I ordered the blue pill, and frictions with mercurial ointment.

20th. Had a stool, in which a portion of the metallic mercury, given on the 17th, was observed.

23d. From the beginning of her indisposition, the patient has had an almost constant difficulty in making water. Sometimes there is a retention, at other times a suppression of urine. For several weeks together, no urine appeared to be secreted. Within the last four or five days, however, the secretion has been restored, but the urine has required to be drawn off by the catheter.

24th. The opium was resumed, in two-grain doses at bed-time.

27th. Although the injections of warm water have been occasionally used since the 20th, the bowels have not been opened since that date. Ordered jalap ʒi. cream of tartar ʒii. to be given immediately, and the blue pill, frictions, and injections to be continued.

May 6th. Since the 27th of April, the mercury has been continued, assisted by sulphate of magnesia, without opening the bowels. During the same interval, the effervescing powders and sweet spirit of nitre have also been frequently resorted to; the former for sick stomach, the latter as a diuretic. The patient appearing to be

sinking, ordered half a pint of brandy, to be taken daily in the form of milk punch.

May 7th. Discontinued the mercury, having failed to affect the mouth. The debility appearing to be greater, increased the brandy to a pint, and ordered volatile alkali. The bowels still remain closed.

13th. Since the 7th, palliatives, stimulants, and nourishing food have been the remedies; such as opium, sulphuric ether, brandy, and beef tea. Since the 8th, the opium has been increased to five grains at bed-time. This day, ordered injections, assafœtida mixture, and frictions with spirit of turpentine, and discontinued the volatile alkali and sulphuric ether.

16th. Reduced the brandy to half a pint and discontinued the beef tea. Examined the caustic issues, and found them reduced to the size of a quarter of a dollar. Continue the dressing with savine ointment.

17th. This day, examined the os tincæ and rectum and was unable to detect any unnatural structure. The bowels remaining still closed, and the secretion of urine suppressed,—ordered the compound tincture of aloes, a table spoonful three times a day, injections, and sweet spirit of nitre.

20th. Discontinued the brandy.

23d. No evacuation from the bowels since the 20th of April. Ordered injections, to be repeated every afternoon. Continue the compound tincture of aloes, sweet spirit of nitre, and the five grains of opium at night.

25th. Bowels still closed. Omit compound tincture of aloes: give ten grains of calomel, three times a day, every other day, and a diuretic mixture of sulphate of magnesia ℥i. and sweet spirit of nitre ℥ii. on the alternate days.

June 4th. No stool. Continue the calomel, and the diuretic mixture mentioned on the 25th ult.

8th. Discontinued the allowance of milk.

15th. Mouth not sore—no stool. Continue calomel and diuretic mixture on alternate days, and use mercurial frictions. Reduce opium to four grains at bed time.

18th. Vomited fæces, with half a pint of liquid. Continue calomel and diuretic mixture. Reduce opium to three grains at night.

19th. Vomited fæces last night and this morning. Reduce opium to two grains at night.

22d. Vomited fæces and liquid.

June 28th. Suspect the calomel has not been taken regularly, the patient having a repugnance to being salivated. Directed the calomel, diuretic mixture, and mercurial frictions to be continued. Omit the opium altogether. The catamenia began to flow yesterday.

30th. Yesterday vomited a small quantity of fæces after the visit.

July 2d. Despairing to affect the mouth, I this day discontinued the calomel and mercurial ointment. Continue the diuretic mixture.

4th. Omit the diuretic mixture, and give a powder of cream of tartar and jalap every hour until the bowels are moved.

7th. Continue the cream of tartar and jalap, and assist its operation by injections of warm water. Ordered six cups to the head, and sweet spirit of nitre.

8th. Omit the cream of tartar and jalap. Continue the sweet spirit of nitre. Give thirty grains of calomel, and tincture of assafœtida, a teaspoonful every hour.

9th. The bowels have not yielded. Stop sweet spirit of nitre and tincture of assafœtida: give spirit of turpentine, a table spoonful every two hours.

10th. Discontinued the spirit of turpentine. Ordered injections of salt and water. Resume the allowance of milk.

11th. Ordered large injections of water.

12th. Vomited fæces mixed with liquid, about a quart. Continue injections.

13th. Last night, vomited fæces mixed with liquid. Ordered effervescing powders.

15th. Ordered thirty grains of calomel, and continued the injections.

16th. Directed fomentations over the abdomen, and a powder of cream of tartar ʒi. and jalap ʒss. to be taken every two hours. Continue injections.

17th. Yesterday, vomited fæces, mixed with liquid, and had two stools, one large, the other small, being the first evacuations per anum since the 20th of April. Continue fomentations and injections of warm water. Apply ten cups to abdomen. Give ten drops of laudanum and two teaspoonfuls of sweet spirit of nitre every hour. Resume the opium, in five grain doses at bed-time.

18th. Had a stool last night, in which something, resembling shreds of membrane, was floating. Ordered half a grain of extract

of elaterium every hour. Continue the laudanum mixture, directed yesterday, and the five grains of opium at night.

July 19th. Had another stool, containing membranous shreds as before. Vomited fæces, mixed with liquid, several times yesterday and last night. Ordered effervescing powders. Omit elaterium.

20th. Vomited fæces this morning, and several quarts of liquid having a urinous smell. Feels no sensation of water in the bladder, but complains of pain in the bowels. No stool to-day. Continue the sweet spirit of nitre, without laudanum. Discontinue the opium by the mouth, and use it as a suppository. Give injections of brine.

21st. Vomited about a pint of fæces and liquid. Continue injections. Stop opiate suppository for one night.

22d. No stool since the 19th. Ordered one drop of croton oil every hour.

23d. Vomited fæces last evening and this morning. Repeat effervescing powders.

24th. Vomited fæces last night and this morning. No evacuation per anum. Ordered five drops of croton oil, to be taken in an ounce of mucilage.

26th. Last night vomited fæces and liquid.

27th. Vomited fæces yesterday afternoon. Continue sweet spirit of nitre, effervescing powders, and opiate suppository.

28th. Vomited last night fæces and liquid, with some blood. Ordered injections, in addition to the remedies mentioned yesterday.

29th. Vomited fæces and liquid this morning.

30th. Vomited liquid and some blood this morning. Catamenia began to flow yesterday. Ordered one drop of croton oil every hour, to be followed the next day by injections.

August 7th. Vomited fæces on the 2d, 3d, 4th, and this date, the remedies mentioned on the 28th ult. having been continued.

9th. Last night succeeded in obtaining stools by injections; the first since the 19th of July.

11th. Vomited fæces and liquid this morning.

12th. Explored the rectum by the finger, and by means of a large catheter, but had no evidence of obstruction.

15th. Was bled to the extent of ten ounces for convulsions. Continue sweet spirit of nitre, effervescing powders, and opiate suppository.

Aug. 16th. Vomited, at two different times, first hard black fæces, and then liquid fæces. Ordered hydrocyanic acid.

18th. Last night vomited fæces, so consistent as to cause the patient great difficulty in forcing them up.

19th. Last night vomited thin liquid fæces.

20th. Vomited thin liquid, coloured with fæcal matter. Milk discontinued. Ordered one pigeon daily, for soup.

21st. Vomited fæces, with liquid, and some blood.

23d. Vomited last night about two quarts of fæces and liquid. Continue sweet spirit of nitre and effervescing powders, and reduce opiate suppository to three grains.

25th. Vomited fæces last night and this morning.

26th. Stercoraceous vomiting recurred.

27th. Reduced the opiate suppository to one grain, at night.

28th. Vomited thin liquid last night and this morning. Ordered fifteen cups to abdomen.

30th. No evacuations per anum since the 9th. Ordered one drop of croton oil every hour, as long as the stomach would bear the medicine, or until the bowels are moved.

31st. Bowels yielded yesterday and this morning; quantity of fæces and liquid discharged per anum estimated at two gallons. Notwithstanding this relief, the stercoraceous vomiting recurred to-day.

September 1st. There being great pain in the abdomen, ordered fifteen cups, and fomentations to the part.

3d. Vomited hard fæces last evening, a quart or more. Continue sweet spirit of nitre, effervescing powders, and opiate suppository.

4th. Increased opiate suppository at bed time to four grains.

5th. Vomited yesterday afternoon four quarts of fæces and liquid. Ordered three drops of croton oil.

7th. Vomited fæces last night and this morning. Continue croton oil.

8th. Took yesterday, and last night, twelve drops of croton oil without producing vomiting or purging. The stomach, however, is very sick.

9th. Last night, after a very severe pain, the patient, while on the close stool, voided something suddenly from the bowels,—its nature not ascertained. Ordered twenty grains of sulphate of zinc,

as an emetic, and directed half a pint of brandy and a pint of milk daily.

Sept. 10th. Vomited half a pint of yellow liquid, of the consistence of pus, after the emetic. The directions of yesterday having been misunderstood, ten cups were applied over the abdomen after the operation of the emetic. The abdomen is softer this morning. The lips are blue, and there exists great anxiety in breathing. Catamenia commenced flowing to-day. Ordered the volatile alkali, and reduced the opiate suppository to three grains. Continue sweet spirit of nitre, and omit effervescing powders.

13th. Within a few days past the patient is incapable of speaking or protruding the tongue, although she appears sensible of what is passing around her. Vomited blood and watery liquid last night. Ordered half a pound of mutton daily, instead of the pigeon, for soup. Continue opiate suppository, volatile alkali, and sweet spirit of nitre.

16th. Passed a restless night. Ordered tincture of iodine, twenty drops, three times a day. Continue sweet spirit of nitre and opiate suppository. Omit volatile alkali.

17th. Ordered injections of a solution of sulphate of magnesia.

18th. After two injections were administered the patient was sick at the stomach. Castor oil was then given, and she vomited two quarts of fæces and liquid. Ordered ol. ricini and injections to be continued daily.

19th. Took half a pint of castor oil yesterday, and used injections. Had the bowels open for the first time since the 31st of August, the stools being thin and watery. Notwithstanding, the stercoraceous vomiting recurred in the afternoon. Continue tincture of iodine, sweet spirit of nitre, and injections. Increase opiate suppository to four grains; omit castor oil.

22d. On the 20th, 21st, and this date, vomited fæces, from half a pint to a pint and a half.

23d. Ordered fomentations, and substituted tobacco injections for those prepared with sulphate of magnesia, one drachm of tobacco to a pint of boiling water, half to be thrown up at a time.

24th. Half an hour after the administration of the tobacco injection yesterday, became very sick at stomach and vomited a pint of fæces.

25th. Continue tobacco injections.

26th. Reduce opiate suppository to three grains.

Sept. 27th. Yesterday, after taking the tobacco injections, vomited about a quart of fæces and liquid. Omit tobacco injections.

29th. Last night vomited about a quart of fæces.

30th. Vomited yesterday afternoon a pint of fæces. Discontinue brandy ordered on the 9th.

October 2d. Vomited fæces yesterday and to-day, about a pint each time.

5th. Vomited fæces and liquid, about a pint.

6th. Rested well last night, although the suppository of opium was not given. Vomited this morning one quart of fæces and liquid.

7th. Vomited last evening somewhat more than a quart of fæces and liquid. The catamenia began to flow yesterday.

8th. Vomited last evening about a quart of fæces and liquid. Has used the tincture of iodine and sweet spirit of nitre at irregular intervals since the 19th ult. also the opiate suppository, with the exception of the night of the 6th. Resume the injections of sulphate of magnesia.

11th. Vomited fæces and liquid on the 9th, 10th, and this date.

14th. I was present to-day for the first time while the patient was vomiting fæces. They were so consistent as to proceed from the mouth in the usual form of healthy evacuations per anum. The patient laboured dreadfully, and appeared almost choked. About this time the tincture of iodine was omitted.

16th. Vomited more than a pint of liquid fæces yesterday afternoon. Between eight and nine o'clock yesterday evening the patient was seized with a violent pain and bearing down. Two syringefuls of water were thrown up into the bowels, and were followed by a hard black stool. Then thirteen syringefuls were administered, and produced a thin yellow stool. Continue the sweet spirit of nitre and suppository; also the injections, to be aided by a pint of castor oil, to be taken in two doses.

17th. Had yesterday morning, after the visit, a stool, consisting of broken-up fæces, about a pint: in the afternoon, another, about half a pint, and thin. Continue sweet spirit of nitre and suppository; also ol. ricini and injections.

18th. Had a small thin stool.

20th. Had a stool last night after the injections.

21st. Had two stools last night and one this morning. About this date, the sweet spirit of nitre was finally discontinued.

24th. Had a stool last evening and one this morning. Discon-

tinue the ol. ricini and substitute the sulphate of magnesia, two ounces daily. Continue suppository, and omit injections.

Oct. 26th. Omit sulphate of magnesia: resume ol. ricini daily. The patient begins to sit up.

29th. Had a stool consisting of broken-up fæces. The suppository of opium, which had been continued in various doses at bedtime since the 20th of July, was finally laid aside on this date. Continue ol. ricini.

30th. Complains of pain in the side. Ordered ten cups to the part.

31st. No stool since the 24th. Ordered, instead of the castor oil, half a grain of elaterium every two hours, until the bowels are moved.

November 2d. Stop elaterium; resume ol. ricini.

4th. Stop ol. ricini; give sulphate of magnesia three ounces.

5th. Had two stools last night, and one this morning.

7th. Took sulphate of magnesia yesterday and to-day in two-ounce doses, and has had four stools since last visit. Gains strength rapidly.

9th. The sulphate of magnesia was repeated.

10th. The allowances of mutton and milk were discontinued, and the patient was put upon the regular hospital diet, consisting of chocolate and tea, with bread for breakfast and supper, and bread for dinner.

14th. Since the 10th, the patient has improved rapidly, and was this day discharged cured.

There are two circumstances connected with this case which I have deferred to mention to this place, in order to avoid tedious repetitions. I allude to the state of the urinary organs, and the occurrence of convulsions. In the narrative of the case, under the date of the 23d of April, the condition of the urinary secretion up to that time is referred to. From that period until the 7th of June there was no evidence of the secretion of urine. During the same interval, no evacuation occurred from the bowels either by vomiting or purging; so that it is not probable that the nurse was deceived. On the 7th of June, the secretion of the urine was restored; but it was retained in the bladder, and required to be drawn off almost every day until the 16th of July, when it appeared to be once more suppressed. From this date, until the 25th inclusive, the secretion was almost entirely suppressed. During this interval, the catheter

was used frequently, either without effect, or with the result merely of obtaining a table spoonful or two of urine. During this period, stools and stercoraceous vomitings occurred; so that the evidence of the suppression of the urine is not so complete, as on the former occasion. Within this same interval, namely on the 20th of July, it will be recollected that the patient vomited several quarts of liquid having a urinous smell, just such an occurrence as might be expected, during a continued suppression of the urinary secretion.

From the 26th of July until the 5th of September, the catheter was used almost every day, and from a table spoonful to a pint of urine drawn off at each operation. From the 6th to the 11th of September the secretion was more abundant, a pint of urine being drawn off each day. It may be worthy of remark, that it was within this interval (on the 9th) that the patient voided something suddenly from the bowels, the discharge of which may have had an influence in increasing the action of the kidneys.

From the 12th of September until the 26th of October, the catheter was used daily, with the exception of three days, and from half a gill to a pint of urine drawn off. On the 27th of October one pint of urine was passed naturally. After that date, the urine was again suppressed for a short period; but on the 5th of November, it once more passed naturally, and has not since been either retained or suppressed. The sweet spirit of nitre, so constantly exhibited during the progress of the case, was directed to the relief of the urinary organs.

The disorder and abolition of the urinary secretion, which were such prominent symptoms in this case, ought not to excite surprise, when taken in connexion with the other remarkable features which it presented. A priori, we might be led to suppose that a long continued suppression of the alvine discharges would cause a diminution, if not suppression, of the secretion of urine, and facts are not wanting which prove this position.

The other circumstance in this case, alluded to above, the mention of which has been deferred, is the occurrence of convulsions. They first occurred on the 26th of May, and finally disappeared on the 15th of October. During this period, the patient was affected with twenty-six distinct attacks, consisting variously of from one to fourteen separate convulsions. These convulsions were often very formidable, and so sudden in their access as frequently to throw the patient out of bed: they were attended usually with long periods of insensibility between the paroxysms.

On reviewing the foregoing case, it will be observed that from the beginning of the attack, on the 27th of January 1827, until the 5th of April, when the stercoraceous vomitings first occurred, the bowels yielded several times imperfectly; namely about the 5th of February, between the 16th and 19th of the same month, and on the 24th of March. Taking into consideration all the circumstances, I think these small discharges per anum proceeded from below the obstruction, being probably feculencies washed out by the frequent enemata employed. On the 13th of April, the first stool was obtained, subsequent to the occurrence of the stercoraceous vomiting, after an interval of obstruction of twenty days counting from the 24th of March, or of seventy-six days reckoning from the commencement of the disease. Stools were then obtained on the 17th and 20th of April. The next occurrence of stools was on the 16th, 18th and 19th of July, after an interval of eighty-seven days. On the 9th of August, stools were again obtained—interval twenty-one days. On the 30th and 31st of August, stools—interval, twenty-one days. On the 19th of September, stools—interval, nineteen days. On the 15th of October, stools—interval, twenty-six days. From this time the alvine discharges took place regularly.

During the intervals above mentioned, the stercoraceous vomitings occurred pretty regularly, and formed a kind of substitute for evacuations per anum. Indeed, the relief derived from the fæcal vomitings was so evident, that on several occasions, when, from a burning sensation in the stomach, the patient was satisfied that the fæces were in that viscus, I gave remedies to promote vomiting. It was with this view that I prescribed the sulphate of zinc on the 20th of September. There was one period, however, of considerable duration, during which no evacuation of fæces either by the mouth or bowels took place. This was the case from the 20th of April until the 18th of June, a period of fifty-eight days. During a large portion of this interval also, namely, from the 23d of April until the 7th of June (44 days), no urine was secreted.

The diet of the patient during her long illness was very various; being sometimes very low, at other times cordial and nourishing, according as symptoms of an inflammatory character or of exhaustion were most predominant. There were periods of long abstinence during the worst state of the stomach from nausea and vomiting, and also intervals of calm, during which the patient ate with considerable appetite. One character of the case was extremely striking, namely, the absence of emaciation. The patient had indeed lost

flesh and the integuments of the extremities were loose and flabby; but there was no attenuation of the limbs, such as is frequently seen in phthisis, and other chronic diseases. I attributed the absence of emaciation to the energy of absorption from the bowels, created by nature's efforts to relieve herself from the load caused by the obstruction.

In casting a glance over the circumstances of this case, and the different therapeutical measures resorted to, it may be profitable to inquire what remedies were most efficient in relieving and finally overcoming the obstruction of the bowels. This inquiry presents many difficulties; as we cannot be certain whether the bowels yielded to the medicines employed, or to the weight and pressure of the fæcal accumulations. The copious stool, obtained on the 13th of April, immediately followed the administration of an enema of tartarized antimony; but the whole effect cannot be attributed to this measure, as elaterium, and cream of tartar and jalap were freely given on the preceding days. The stools produced on the 17th and 20th of April appeared to depend on the administration of the metallic mercury. Next occurred the longest interval of obstruction, reaching to the middle of July, when the evacuations occurred, containing shreds of membranous matter. No doubt these were portions of the internal coat of the intestines, which had separated by sloughing. Still however the obstruction was not removed. The discharges per anum, mentioned as remarkably copious, occurred on the 30th and 31st of August, after repeated doses of croton oil, and may be supposed to have given the greatest relief; and on the 9th of September, the expulsion of something suddenly from the bowels took place. I regret exceedingly that the nature of this substance was not ascertained; but may we not conjecture that it was a portion of intussuscepted intestine, which had been detached by sloughing? Assuming this conjecture to be probable, it still is difficult to explain why the obstruction did not yield from this moment. Nevertheless the obstruction recurred. On the 19th it again yielded to injections and castor oil, and was finally and permanently relieved by the same remedies on the 15th of October. Upon the whole, I should be inclined to believe that injections of warm water, to act by its *quantity*, and the croton and castor oils were in this case the most efficacious remedies.

Since the occurrence of the above case, I have been desirous of ascertaining whether any analogous one is on record; but my time

has not permitted me to make the necessary researches. I have however, in my reading, recently met with several cases possessing some points of resemblance. In the Dublin Hospital Reports, Vol. IV. 1827, Dr CRAMPTON relates several cases of long continued constipation, the principal one of which has several characters in common with the case detailed in the present communication. In Dr CRAMPTON's principal case, that of a female, there occurred frequent vomitings; the matter ejected being sometimes stercoraceous, and sometimes urinous both in taste and smell, while but little urine was "either secreted into, or passed from the bladder." The patient vomited every day; the intervals between the occurrence of stools became longer, and the urine was seldom passed. In this case, like mine, the catamenia occurred. In 1827, the patient had been in this state for *seven years*, and for the last eight months of this time "had no passage from her bowels, and only two or three during the preceding year."

Dr CRAMPTON next relates the case of Anne Free, in which "both the bowel and urinary discharges were nearly suppressed." She never passed urine, except when relieved by the catheter;" and was affected with stercoraceous and urinous vomitings. This patient lived for several years.

In the Bulletin des Sciences Medicales for March 1828 there is related a case taken from a German periodical, as drawn up by Dr BERRES, professor of pathological anatomy at Lemberg, which tends to show the connexion between obstinate constipation and the suppression of the urinary secretion. The subject of this case was a boy, thirteen years of age. On the 22d of October 1822 he was seized with fever, which, with another attack occurring during convalescence from the first, left him in a state of great debility. He had an obstinate constipation which lasted for ten or twelve days. On the 20th of November the patient was seized with violent pains in the rectum, which were much aggravated by efforts at stool. In January 1823, the constipation continuing, the urine was observed to be diminished, and its secretion became suppressed for fifteen days. Two similar suppressions occurred between this period and March, when a third took place, and continued for six months.

In July 1823, the patient was in an advanced state of marasmus. The constipation continued, and he was affected with a periodical pain in the loins, of such violence as to cause him to lose all sense and to become affected with convulsions. The belly was distended

and elastic, and the bladder appeared to be empty. After the lapse of six weeks from this period, the urine began to flow abundantly, in consequence of a paroxysm of fear, brought on in the patient by the instrumental preparations of his surgeons. From this moment the patient recovered.

It is time to close this communication, already extended to too great a length. It may be worth while however to mention, that up to the time of my writing (20 August 1828) my patient has continued in good health, having long since regained her flesh and strength.

ARTICLE VII.—*Observations on the Removal of Foreign Bodies lodged in the Œsophagus. Read before the Philadelphia College of Physicians* 30 *July* 1828. *By* HENRY BOND, M.D.

[With a Plate.]

There are few medical practitioners who have not been perplexed with cases of a foreign body lodged in the passage from the mouth to the stomach. And it must be admitted, that the subject has been treated of by surgical writers more vaguely than would seem to us consistent with its importance; whether we regard, on the one hand, the surgeon's perplexities and failures, or, on the other, the patient's distress and danger. Cases often occur where the result is fatal; and perhaps there is no one where the practitioner is oftener unsuccessful in fulfilling a clear indication. This is not, I apprehend, owing to any intrinsic and insurmountable difficulty; and had it received as much attention as some others of not more importance, the practitioner might be charged with mal-practice, who should fail of giving prompt relief to his patient. Some who have written systems of practical surgery have wholly omitted the subject. One reason of this neglect perhaps may be, that in the treatment of these cases reputation is neither acquired nor lost. They are much less attractive to popular admiration than those cases where there is a display of knives, and gashes, and blood. As one proof of this, it may be remarked, that the dealers in terrible

accidents and operations seldom deem these cases worthy of a place in their chronicles.

Foreign bodies are usually arrested so high in the fauces or pharynx, that they are visible upon depressing the tongue, and looking into the throat. In such cases relief is usually afforded without difficulty by means of the fingers or a dressing forceps. But when the body has passed into the œsophagus, beyond the reach of the eye and the finger, the difficulty is much increased. The difficulty and danger of the case will also depend very much upon the size, form, and composition of the body. In some instances if relief be not afforded by the prompt removal of it, the patient is suffocated, and perishes immediately. Does this result depend upon the mechanical obstruction to the trachea, or upon the spasmodic constriction of the muscles of the glottis? Mr Charles Bell says, (and the opinion was adopted by Dr Dorsey) that, "it is not the mechanical obstruction to the trachea which we have to dread from bodies sticking either in the trachea or œsophagus; it is the spasmodic contraction of the muscles of the glottis." After enumerating all the substances which are liable to lodge in the throat, he says, "*all these* endanger suffocation by exciting the muscles of the glottis." This opinion may be true so far as it applies to foreign bodies in the trachea; but as it regards the pharynx and œsophagus, it is confirmed neither by my observation, nor by the cases which I have found recorded, or which have otherwise come to my knowledge. In nearly or quite all of those cases where immediate suffocation takes place, the body is of sufficient magnitude to produce a mechanical obstruction. The patient, through fright or voraciousness, attempts to swallow a large piece of tough meat, or other substance; the epiglottis falls down; the foreign body descends upon it, and becomes impacted between the horns of the os hyoides and those of the thyroid cartilage, where it is held by the efforts of the muscles of the pharynx to carry it down. Where there is a spasmodic closure of the glottis produced by the irritation of a foreign body, there will be a spasmodic effort of the chest to throw it off by coughing. If coughing could be induced in the case we are contemplating, there is very little doubt but the body would thereby be dislodged. But the mechanical pressure upon the epiglottis prevents the inspiration, without which coughing cannot take place, at least with any considerable impetus.

When suffocation takes place, in consequence of the inflammation

and tumefaction caused by the pressure of a foreign body, the result is not attributable to the irritability of the glottis, but to the mechanical obstruction produced by the swelling. Where death occurs suddenly, after the lodgement of a large body at the top of the larynx, the glottis does probably contract spasmodically; but if the body did not act mechanically, in the manner above described, coughing would be induced, and the body dislodged. A barb of rye or wheat, or other small body, may become fixed in or near the glottis, and irritate so much as to lead to a fatal result by coughing or otherwise; but I know of no instances where suffocation has been produced by pins, needles, fish bones, or the like, irritating the glottis to contract spasmodically.

These views seem to me to be legitimate deductions from the anatony and physiology of the parts, and from the history of cases as far as they have come to my knowledge. But I forbear any further discussion of this point, as I began this communication with the intention of making only some practical remarks on the means and methods of treatment. This will necessarily vary, according to the properties of the body, and the part of the canal in which it is arrested. This most commonly takes place in the middle or lower part of the pharynx, but it may occur at any point between the palatine arches and the cardia. If the body be large, it will not descend to the narrower or lower part of the pharynx, but will be fixed at, or near the top of the larynx. It is the lodgement of a large body at this part, which occasions nearly all the sudden deaths which occur from foreign bodies lodged in this canal. Death occurs so suddenly in these cases that medical aid is seldom obtained until the patient is beyond its reach. Were a person at hand at the occurrence of the accident, who understood the nature of the case and the remedy, relief would be easy and certain; for in these imminently dangerous cases, the body will be found, as before stated, locked between the cornua of the os hyoides and thyroid cartilage. From this position it may be removed by the finger or by the common dressing forceps, if the curved forceps are not at hand. If the foreign body be in any other part of the pharynx, there will be much less immediate danger.

In most cases where a foreign body is to be removed from the throat by the fingers or by other means, it would be advisable to put a piece of soft wood or cork between the teeth. This advice is, however, of less importance to be observed, than that of Mr C. Bell, who says, "we ought not in an impressive and rapid manner

to seize upon the patient; for this puts him into terror and anxiety, that increases the difficulty of breathing. Without much loss of time, it is possible to sit down before him in that composed manner which will give him confidence of relief."

If the body is to be removed by the fingers, let the patient's head be erect, and held in such a manner as to make the chin project as little as possible beyond the pomum Adami. This will enable the operator to carry his finger considerably farther into the pharynx than when held in the natural position, and he can with ease reach that part where the body lodges. If the body descend beyond the reach of the finger when held in this manner, it will be too low to endanger an immediately fatal result, unless it lodge directly opposite to the top of the sternum. By pressing on the posterior part of the thyroid and cricoid cartilages, a large foreign body may produce some difficulty in respiration, but very seldom to a dangerous extent, as those cartilages have so much firmness, that they would be shoved forward or to one side, before they would be so compressed as to stop respiration. The trachea is much more easily compressed, but it is also surrounded by parts so loose and lax, that a foreign body in the œsophagus would displace the trachea, before it would compress it so as to interrupt respiration, except at the top of the sternum, where it might take place to a dangerous extent. This view is supported by a case recorded in the 3d volume of the Dublin Hospital Reports. Something had stuck in the throat of an aged woman while eating, which nearly produced fatal suffocation; and on inquiring where she felt the distress, she pointed to a particular part of the throat which appeared to be directly *opposite to the top of the sternum*. Mr Howship "passed a probang down her throat to the piece of meat forming the obstruction, and pushed this substance *nearly down* into the stomach, when relief was immediately obtained, it being evident that the mechanical pressure of the trachea had been the sole cause of the severe symptoms." I might, with propriety, have referred to this case as confirmative of the views which I advanced when examining Mr Bell's opinion of the cause of suffocation. The efforts of the finger may be aided when the body is large by grasping the trachea and œsophagus with some firmness immediately below the point where the body rests, and at the same time shoving the hand upwards. This grasp, for obvious reasons, should not be long continued. If the body cannot be removed by means of the finger alone, let the tongue be depressed by

the fore finger of the left hand on a spoon handle, and the operator will be able to see the body and grasp it with a forceps. If a gula-forceps is not at hand, the common dressing forceps will answer the purpose. Any person with self-possession and an ordinary share of manual dexterity might be very readily taught how to afford relief in such cases, although possessed of no anatomical knowledge or surgical skill.

Besides the larger bodies, to which the preceding remarks have more special reference, other bodies, such as needles, pins, bones, &c. may lodge in the fauces or pharynx without endangering suffocation. These cases are, however, not always free from difficulty and danger, as may be seen by referring to baron LARREY's account of the case of M. Esteve*. The difficulty in such cases arises principally from the smallness of the body allowing it to be concealed in some sinus. This at least was the fact in the case referred to; but there are, I apprehend, few such lurking places about the fauces, which could not be explored by the finger or the eye.

When the foreign body descends into the bottom of the pharynx, or into the œsophagus, although the immediate danger is lessened, the operator's task becomes more difficult, and the difficulty increases in proportion to the depth to which it descends, unless the body be such as may with safety be forced into the stomach by means of a probang. So great, indeed, has the difficulty been considered, that the idea of extracting a body from the œsophagus through the mouth by instruments seems to have been almost hopeless among surgeons. When a body has passed out of sight, or out of the pharynx into the œsophagus, Mr B. BELL says, "No advantage can be derived from the forceps; and we are in such circumstances reduced to the necessity of either allowing it to remain where it is; of pushing it into the stomach; or of extracting it by laying the œsophagus open." "In every case of obstruction of the œsophagus," proceeding from a foreign body being fixed in it, he says, "it is almost the universal practice to endeavour to push it into the stomach."

Mr BELL indicates the cases where he thinks this would be "the most prudent method of getting free from it;" at the same time pointing out the danger of the indiscriminate use of the probang, as

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* See HALL's translation of LARREY's Memoirs of Military Surgery, Vol. I. p. 142.

well as the mischief which may result from the use of strong vomits. It will be seen by referring to his Surgery, that he never contemplated the removal of a foreign body from the œsophagus by forceps, or any other instrument except the scalpel; and we are directed to make no attempts to remove it unless the symptoms are very urgent. We must wait for the body to decompose, or until the œsophagus ulcerates enough to give it an outlet. If however the symptoms are too urgent to admit of this delay, we must, as he thinks, proceed to œsophagotomy. Mr ALLAN BURNS entertained nearly the same opinion as Mr B. BELL of those cases where a foreign body gets below the pharynx. "But if it stick just at the top of the œsophagus," he says, "it is there too low to be laid hold of by the fingers; and even the curved forceps can hardly be so applied as to extract the foreign body; neither will the probang enable us to force it into the stomach; or granting that it would, we may have reasons for not wishing to place it there." If, in such a case, the foreign body occasions very unpleasant symptoms, he says, "there ought to be no hesitation in performing the operation of œsophagotomy." It was the opinion of Dr DORSEY (which we presume was also that of Dr PHYSICK) that foreign bodies can be extracted through the mouth, only when they are situated high up near the fauces. "If near the stomach, they must," says Dr DORSEY, "be pushed down, and the risk of the consequences incurred."

Some surgeons, on account of the dread which they attach to the operation of cutting into the œsophagus, which they think should almost never be performed, have proposed several instruments or contrivances for the extraction of foreign bodies from this canal. The success of practice in these cases shows that the patient is very often obliged to trust to *vis medicatrix naturæ*, rather than to these contrivances for his safety. I shall first notice in a brief manner the means which have been proposed for extracting foreign bodies from the œsophagus, and then propose some of my own, which I venture to hope may be considered improvements in the treatment of such cases.

The instruments which have been recommended, or are now in use in such cases, are forceps, hooks, compressed sponge, and loops of thread or horse hair.

It is hardly proper to include the *forceps* among the instruments employed for extracting foreign bodies from the œsophagus, as I have already shown that it is generally concluded to be inapplica-

ble to such cases. The crooked, or gula-forceps (fig. 2) is well adapted for the removal of large foreign bodies from the pharynx; but for extracting bodies from the œsophagus, it is defective in several respects. It is too short; its curve is such that it would be next to impossible to carry it down that canal if it were long enough, being curved in such a manner as to be accommodated to the course of the canal when the head is held in a natural position; the blades come into opposition by broad surfaces, terminating in an acute prominent edge, like two half cylinders; so that when the instrument is passed down the throat we are almost sure to pinch some of the surrounding soft parts, if we attempt to grasp any small body; if we grasp a pin or needle, the teeth or deep transverse grooves in the blades render it necessary either to bring out the body transversely, something like dragging a fish-hook through the fauces, or to run a great risk of losing our hold of it. On referring to a large volume of plates of surgical instruments, published at Vienna, by G. A. Brambilla, I find some crooked forceps considerably longer and heavier than those now in use here and in England, but liable to the same objections, and from their shape evidently not intended to be carried below the pharynx. The crane-bill forceps, recommended by Mr C. Bell, is exempt from none of the foregoing objections. In short, we may say that these instruments cannot be trusted out of sight. The forceps recommended by Mr J. Hunter for extracting foreign bodies from the urethra, has been recommended also for extracting small bodies from the throat. It would be less liable to pinch the contiguous soft parts, but in every other respect it seems to be decidedly inferior to the common dressing forceps*. The *ostagra* of professor

* In the Medico-Chirurgical Transactions, Vol. XI. page 359, there is an account of a modification of Mr Weiss's bullet forceps, by which it was converted into an instrument for the extraction of small calculi from the bladder. Mr Samuel Cooper, referring to this instrument in the fifth edition of his Surgical Dictionary, says "no instrument seems better calculated for cases in which the body lodged in the œsophagus is not too wide." It does not however appear to me superior in any respect to Desault's modification of Hunter's forceps, either for extracting urinary calculi from the bladder, or foreign bodies from the œsophagus. And, by the way, we cannot forbear to express our surprize, that Sir Astley Cooper should have been ignorant of the practice of Desault so late as 1820, six years after it had been circulated in the United States in an English translation. In size and shape, the forceps above recommended by Mr

VENEL was contrived to be employed in these cases; but it is a complicated awkward instrument, whose place can, in every instance, be better supplied by some other.

Every writer at present, who treats on this subject, recommends the use of a hook. It was formerly the custom to attach a small steel hook to the handle of a probang, or to a piece of whalebone, by means of a piece of watch-spring. But it is of late commonly recommended to be made of a doubled and twisted wire, with a loop at the end bent into a hook. We find a representation of its figure in some surgical works, and nearly the same in each. Fig. 5 is the same as that in Mr S. COOPER's First Lines, and does not differ essentially from that represented in Mr C. BELL's Operative Surgery. There are very few cases to which such a hook would be well adapted. It is ill suited to extract a coin, as such a body would be so liable to slip out of it. It would take no hold of a pin, needle, or fish-bone; for when these bodies are lodged in the œsophagus, they have descended point foremost; the point catches in some ruga or lacuna, and the efforts made to carry them down into

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S. COOPER, has almost an exact resemblance to a large male silver catheter bisected longitudinally about three inches and a half from its point. These two halves or sections are opened by a stilet sliding in the inside; and are closed only by their own elasticity. Its curvature will not allow it to pass much below the bottom of the pharynx; and if it should be forced lower down, the point would press so hard against the anterior part of the œsophagus, that there would be very little chance of seizing the body, and every probability of embracing a large fold of the canal in closing the blades. There is no way to regulate the force applied to the body which it embraces, and it must be small, or that stilet would not open it. The blades form so large an angle with each other when opened, that no body would be held securely except the small calculi, for which it was intended, or other small globular bodies. As an œsophagus forceps, it is therefore in no respect superior to the common curved forceps. It is true, it has no joint; but when a forceps is carried down into the œsophagus, there is so little motion in the joint when the blades are opened or closed, that there is not much probability that the soft parts will be injured there.

In PERRET's *L'Art du Coutélier*, plate 98 is the representation of a forceps designed for the removal of foreign bodies from the œsophagus. It is called *acanthobalon* by PAUL, HILDET, and others. Excepting its curvature, it is made exactly like the common bullet forceps. The instrument is straight until we pass an inch and a half beyond the joint. The rest of the blades are bent so as to correspond nearly with the quadrant of a circle whose diameter is eight inches. Its curve is therefore more favourable to a deep introduction into the œsophagus than any other of which I find any account. Its defects may be perceived from what I have already said of others.

the stomach have often driven the point into the side of the canal, so as to adhere with considerable firmness. The upper or obtuse end of these bodies is generally loose or floating; that is, it is not fastened into the opposite side of the œsophagus. This doubtless is not always the case, but how often both ends are fastened in opposite sides of this canal it is not easy to determine. Under either of these circumstances there is not a great probability that such a hook as that represented by Mr Cooper would dislodge one of these bodies, and none that it would extract them by the mouth. All that Dr Dorsey says upon the use of hooks, is, "that a hook of curved wire is also useful in some cases for extracting foreign matters, especially when they are of considerable size." Upon this opinion, unsustained by experience as far as I know, it may be remarked, that if the body has lodged in consequence of its size, it would be very difficult to pass a hook below it, especially if the hook were large enough to take hold of "a body of considerable size." There is therefore every probability that a hook of the ordinary form would act as a probang. Besides, let a person attempt to apply such a hook to any body of a regular or rounded form, or a smooth surface, such as a nut, a peach-stone, a lump of meat or potatoe, and he will in a moment see how improbable it is, that such a hook would extract such a body. If the body to be removed had any holes or projections upon which the hook could fasten, and would at the same time allow the hook to pass below it, in such a case the hook might be useful; but under this restriction its use must be very limited.

Mr C. Bell says, "Nothing is so good to pull up bodies from the œsophagus and pharynx as a piece of wire twisted and bent into a hook, as I have represented in the plate. The advantage of this simple instrument is, that it can be made of any form, or take any curve, enabling us to adapt it so as to operate upon that side where the pin or bone is supposed to lodge." We admit the superiority of the wire-hook over others and for the reason he has assigned; but we are extremely doubtful of the ability of Mr Bell or any one else "*to pull up* from the œsophagus and pharynx" either a pin or stone with the hook he has represented.

For the extraction of a pin from the œsophagus, he has proposed to carry loops of horse hair down to it on the end of a wire through a hollow tube, in the expectation of entangling the head of it. The probability of the success of this contrivance is sufficient to warrant

a trial of it, when other more certain means are not at hand. For the extraction of needles and fish bones, he offers no rules nor means which encourage us with any prospect of success. Neither his forceps, nor his hook, nor his loops are applicable to such cases.

Mr S. Cooper's ingenious method of using compressed sponge seems to me more extensively applicable, and to hold out a greater prospect of utility, than Mr Bell's loops of hair. It would, however, much more frequently dislodge small bodies that become fixed, than extract them through the mouth. One objection to it is, that if the first trial were not successful, it would require a considerable time to prepare the sponge to repeat the experiment.

It is easy to conceive of cases where it would be hazardous to resort to vomiting for the removal of foreign bodies; for they may have such sharp points or edges, or raggedness, that we should run a great risk of cutting or lacerating the passage by their violent action. A very small portion, however, of those bodies, which lodge in the œsophagus, have a form which precludes the use of emetics. They promise to be more certainly useful, where the body is of a regular or rounded form and smooth surface, and is arrested in the passage on account of its magnitude. If a small body, having only one rough or sharp point (such as a pin, or one half of a round bone of a bird or chicken), be arrested in the passage, we may always presume that it has descended point foremost. In such cases emetics may be used with impunity; but, if the descending point be so slender as to penetrate with ease the side of the canal to a considerable depth, we shall use them with small prospect of success. Such a piece of bone as that just mentioned would without much doubt be thrown up, as the point would not penetrate far, and it would be of such a size, that both the contents of the stomach passing up, and the inverted action of the canal would have some power over it. When the sharp slender point of such a body as a pin or needle penetrates the side of the œsophagus, the direction of the body is as nearly transverse to that of the canal as its length will admit of; but when an emetic operates, the pin is brought nearly parallel to the axis of the canal, which will occasion the point to stick more firmly, and at the same time neither the contents of the stomach passing up nor the inverted action of the œsophagus can act with any force upon it. Hence pins can seldom be removed by this means, especially if they have been lodged a considerable time, and attempts have been made to carry them into the stomach. I have repeatedly employed vomits in such cases, and

never with success; but I once removed part of the clavicle of a chicken immediately by the use of sulphate of zinc. The above remarks will apply to a needle, except for the circumstance that the two ends of it, especially if it be small, may fix in opposite sides of the canal. In such a case, an emetic will cause the body to be more firmly fixed; but if the needle be long and large we need not dread this accident, for it will remain nearly parallel with the course of the canal. An irregular rough fragment of bone would perhaps never be removed by the mere inverted action of the œsophagus, but it might sometimes be forced up by vomiting, if this should be induced soon after swallowing a considerable quantity of solid food. The success attending the use of vomits for the removal of *coins* from the œsophagus is not such, so far as I can learn, as to encourage a resort to them. A case occurred within a few days in this city, where vomits were repeatedly tried without success in the case of a child who had swallowed a half cent. After a coin has been in the œsophagus for some time, the part where it lodges has, from continued distention, lost its contractility; the parts above and below contract, perhaps the more on account of the neighbouring irritation; a sort of pouch or sinus is formed for the foreign body, and, as the natural position of the coin would be vertical, there would be space enough for the contents of the stomach to pass up without dislodging the coin.

Two cases are quoted in the Dict. des Sciences Med. Tom. VII. p. 21, 22, in which persons, at the very point of suffocation from the lodgement of large pieces of tough meat in the pharynx, were relieved by injecting a solution of tartarized antimony into a vein in the arm. In Dorsey's Surgery two cases are noticed, in which Dr Physick removed peach stones from the pharynx by directing the patients to take a solution of tartarized antimony in the mouth and to attempt to swallow it. These and other similar cases show that it would be very unwise to discard emetics from among the means of treating such accidents.

Another means of relief deserves to be noticed, as it has succeeded in cases where other means failed. As professor Richard W. Hall, of Baltimore, is the only writer by whom I find it mentioned, I will present it in his own words. It is contained in a note to his translation of Larrey's Memoirs of Military Surgery, Vol. I. p. 143. "When a small bone is lodged in the fauces or œsophagus, it may be decomposed, or rendered so flexible that it will pass into

the stomach, by taking frequently of diluted mineral acids. They should be taken through a tube to prevent their action on the teeth. The strength of the acid should be accommodated to the sensibility of the parts over which it must pass. By this means I have succeeded in removing a small chicken bone from the œsophagus, across which it had remained firmly fixed for several hours, although an emetic had been administered, and the curved forceps and probang had been repeatedly used without success. Experiment shows, that even the acetic or acetous acid will decompose the bones of fish, and leave them in a pliant and gelatinous state."

Although forceps have never been carried deep into the œsophagus, a slight attention to the subject is sufficient to satisfy any one, that there is nothing in the structure of this passage to prevent the introduction of an inflexible instrument to any depth. I have already pointed out the defects, as they appear to me, of all the forceps which have been recommended for the extraction of foreign bodies from this passage; and these defects may all be traced to two erroneous ideas, viz. 1, That a forceps cannot be trusted below the pharynx for fear of pinching or lacerating the soft parts; and 2, that the bend of the instrument must be adapted to the course of the passage when the head is held in its natural position. While held thus it would be impossible to carry an inflexible instrument to the bottom of the œsophagus; but by carrying the head back to no dangerous or painful extent, even a straight instrument may be carried down into the stomach, as is evinced in the performances of jugglers, who swallow swords, &c.

I have caused a forceps (fig. 1) to be made, which seems to me to possess some advantages which do not belong to any other.

1. I have given it such a curve, that when the head is held back in a proper position, it may be introduced into the œsophagus to any depth, even into the stomach. Whether in this first attempt I have succeeded in giving it that curvature which upon further trial shall be found best, I cannot assert; but my object was to make it such that it would pass down with facility when the head is carried back, so as to make the axis of the whole passage from the mouth to the stomach approximate as near to a right line as could be done without giving the patient inconvenience. In some trials with it, I found it could be introduced with great ease. An inexperienced hand would be less liable to carry its point into the larynx, than that of the common curved or gula-forceps. Should experience hereafter

indicate any change in its shape, it probably will be to make it less curved.

2. The next advantage which it possesses, is, that the blades may be opened and closed in the œsophagus without pinching or otherwise injuring the contiguous soft parts. This object is fully accomplished by the manner in which the edges are ground or beveled off, and by the sheath of elastic gum which embraces the joint without impeding its motion. The blades of the forceps are so long and can be opened only so short a distance in the œsophagus, that there will be very little motion at the joint, and in almost every case I should therefore consider the sheath superfluous. Should any one consider the instrument unsafe without it, it should be a little longer than the joint, and may be made by sewing with the glover's stick a thin piece of gum elastic, of the proper dimensions; or it may be made more neatly by adopting the method for making a tube, which is recommended in Ure's Chemical Dictionary. (See Art. *Caoutchouc.*)

3. Another advantage is, that by means of the narrow line of rough surface by which the blades come in contact, it will hold such a body as a pin, needle, fish-bone, &c. more securely than the common curved forceps, and extract it with little or no danger of lacerating any part of the passage. At the same time that it holds the body fast, it allows it to vibrate freely, and to assume a position nearly parallel to the blades. For the extraction of a coin its adaptation seems to be perfect. In short, it appears to be more especially adapted to the removal of those bodies which are arrested on account of their form, and not on account of their volume. When a body, without any sharp points or projections, such as a chesnut, a small apple, or piece of meat, is lodged in the œsophagus, we may presume that the canal, at that point, will be so distended that there will be some difficulty in passing the blades below the body. This probably will seldom be practicable where the body has been forced down by the bold application of the probang. But where the body has been carried only so far down as it could be done by the action of the muscles, it is probable that by opening the blades so as to distend the œsophagus considerably, just before they reach the body, they might be carried below it. By opening the blades in this manner they would necessarily pass down on opposite sides, so as to bring the body between them, and embrace it securely, unless it were vitreous or very hard and polished. But in most cases of

large, regular, or smooth bodies lodging in the œsophagus, if there be any difficulty in extracting with instruments, we may resort to emetics with very strong hopes of success; for it is to such cases that they are more especially adapted.

Although I believe there are few cases where the need of a hook will not be superseded by the forceps here presented to the profession: yet, as I have pointed out the defects of such a hook as we find in the books, I take the liberty to offer some modifications, as hooks may be made extemporaneously when a forceps is not attainable. The hook fig. 3 is intended more especially for the extraction of pins. It may be made of doubled wire, but I should prefer it single, similar to the plate. A single wire is sufficiently firm for all the force it would be prudent to employ; it presents a smoother surface to the soft parts over which it passes than a doubled and twisted wire; it is more easily bent so as to act in different directions; and perhaps there would be some small difference in its favour in the facility with which it would catch the body sought for. If it should be thought that the lower end of the hook, on account of the shortness of the bend, presents too sharp a point to be passed with safety down the œsophagus, cover the point with a drop of sealing wax. This precaution would be needless in hands of ordinary dexterity,—and destitute of it, no one ought to meddle with surgery. The space between the ascending and descending parts of this hook should be accommodated to the size of body to be extracted. In general it should be such as to admit the body of a brass pin with ease, without allowing the head to slip through. The two parts of the hook should be parallel to each other, about half an inch upwards from the bend; otherwise there will be danger, as soon as we have caught hold of a pin, that it will slip out and pass into the stomach, as I have repeatedly experienced; and, instead of extracting, we shall only dislodge it. With this hook there would be a better chance of dislodging, if not of extracting, a needle than with any other; but even with this it would be a doubtful experiment, and time ought not to be wasted where the forceps are procurable. The softer texture and more uneven surface of most other long and slender bodies render it much more probable that they could be extracted by this hook. To prevent the escape of a body from this hook, a very simple addition to it might possibly be found useful in some cases. I do not propose it, however, with much confidence in its utility. Let a wire, similar to that of which the hook

is made, be bent at one end into a ring, just large enough to slide upon the handle of the hook; and let the wire to which this ring belongs slide through another ring at the top of the handle of the hook. By dropping this sliding ring upon a body, when caught in the hook, there might, in some cases, be a greater probability of extracting it.

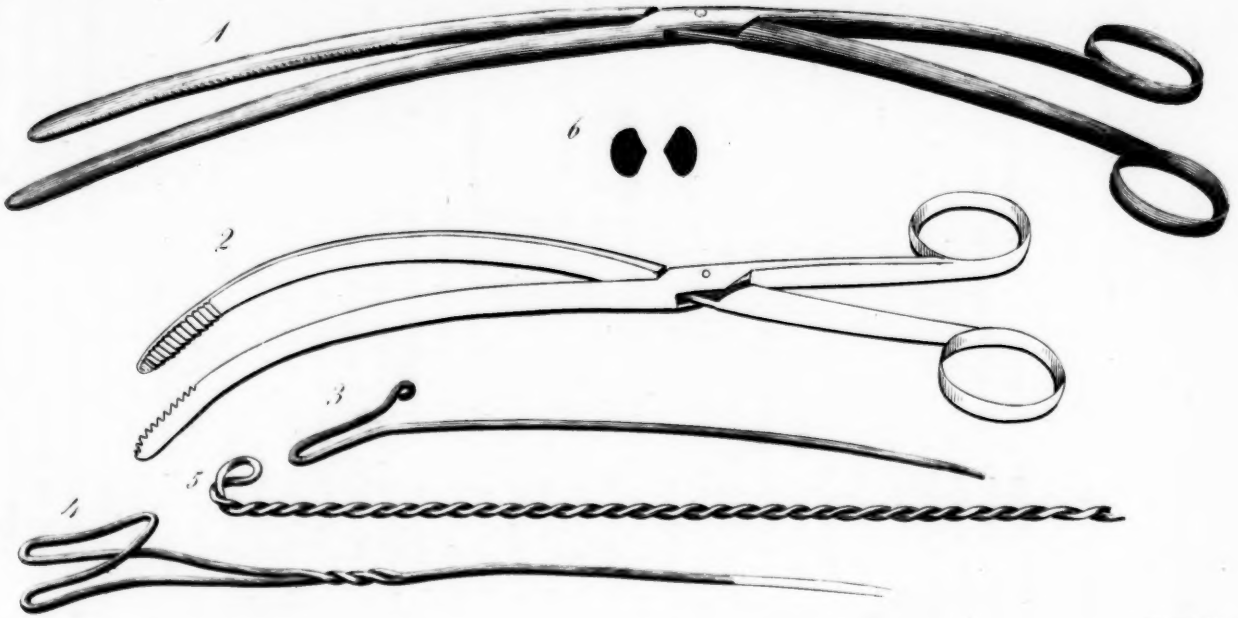
For the extraction of a coin from the œsophagus, the lower part, if not the whole of the hook (fig. 4) should be a doubled wire. The descending and ascending parts of it should be only far enough apart to admit the coin with ease, and the two parts of the wire at the bend should be so far apart that it would not be apt to roll out. The descending and ascending parts of the hook should be parallel to each other, so far as to sustain the coin in a vertical direction; but the point should stand off so far from the handle as to make it easy to catch it. A hook made in this manner would have some advantage over that which was presented to the profession two or three years ago by Dr NATHAN SMITH of New Haven. A coin would be less liable to escape from it, on account of the much broader resting place for it at the bottom; the opening of it is such that it would more readily take hold of the body; the handle may be readily bent so as to act in any direction; and it may be made extemporaneously out of a piece of common wire.

Although a probang may in some instances be used with safety, I apprehend that surgery would sustain no essential loss if it were entirely discarded from its apparatus; for in every case where it would be applicable, the patient would be relieved with more ease and safety by the forceps, or by vomits. In those cases to which a a hook is applicable, the probang should seldom or never be employed. Whether, therefore, it be advisable to retain among our apparatus an instrument which is never necessary, and which is so liable to be used in improper cases, it is not for me to decide; and whether any thing contained in this communication be entitled to the distinction of an improvement in surgery, I leave to be determined by this very competent tribunal, and by a profession whose test is experiment.

Explanation of the Plate.

Fig. 1 is drawn from a forceps twelve and a half inches in length, which will reach two or three inches below the top of the sternum. For reaching the bottom of the œsophagus in the adult, the instru-

Bond's Oesophagus Forceps



W.E. Mason del.

C. Cooper Sculp.

ment should be two to four inches longer. The curve corresponds nearly to a circle, whose diameter is twenty-four inches. It does not, however, exactly correspond to a circle, for the curvature lessens in passing from the handle to the point. The joint should be distant from the point not more than three-fifths of the length of the instrument, and be made long, in order that the blades may with more certainty come exactly in opposition. The edges of each blade are ground away with such a bevel that the blades present to each other, where they come in contact, an obtuse angle of about 130 degrees. This angle on each blade is made rough by cutting it into a row of teeth, either by a chisel or a file. The point of each blade is ground off like the edges, so that the row of teeth is not continued quite to the point. This figure is half size.

Fig. 2 is an outline of the common curved or gula-forceps, presented here to illustrate its defects by contrasting it with my own. The curvature commences two inches beyond the joint; and from that point it forms the sextant of a circle, whose diameter is six inches. This might with propriety be named the *fauces forceps*. It is half size. See page 284.

Fig. 3 is the hook which I propose for the extraction of pins, &c. It is of the full size, and sufficiently explained at page 291.

Fig. 4 is the hook proposed for the extraction of coins from the œsophagus, and is of the full size. See page 292.

Fig. 5 is copied from the hook represented in Cooper's First Lines. See page 285 of this communication.

Fig. 6 represents a transverse section of the blades of the forceps fig. 1.

Article VIII.—*Observations on Cancer of the Breast. By* Joseph Parrish, M.D., *One of the Surgeons to the Pennsylvania Hospital.*

In a former paper I gave my views relative to certain morbid conditions of the breast which might be mistaken for scirrhus:—in the present my object is to make a few observations upon the genuine disease, as it appears in that organ.

It usually commences with a small, distinct tumour; which in some

persons runs its course with great rapidity; in others, remains indolent for a long time, for months or years, sometimes for many years. When it becomes active, the first symptom is generally a lancinating pain shooting through it. The patient also sometimes complains of an exceedingly distressing, burning pain. The tumour increases in size, and, as it augments, embraces the neighbouring parts, which it identifies with its own peculiar structure. When it reaches the surface, its character becomes more obvious. There is commonly a wrinkled or puckered state of the skin, which I consider as one of the strongest evidences of the cancerous nature of the affection. The skin also changes colour, becoming of a dark red; and ulceration at length takes place. The ulceration, so far as I have observed, is superficial. There is no abscess, nor genuine pus, the discharge being of an ichorous nature, and exceedingly offensive. The odour of the secretion from cancerous ulcers is peculiar, as much so as the mercurial fœtor, or that of small pox. An experienced surgeon will often be able to recognize the complaint upon entering the door of the chamber. Hectic fever now comes on, attended with great nervous irritation and extreme suffering. Sometimes the complaint extends backward, involving the fascia of the pectoral muscle, and the muscle itself; and the tumour no longer remains moveable as when the mamma alone is affected, but becomes fixed by union with the parts beneath it. The absorbents also take on disease. The glands in the axilla enlarge; the disordered action extends to the glands of the neck, and travels into the chest; and, in some cases, almost the whole system is affected before the patient is relieved by death.

I have stated that the complaint occasionally passes into the lungs. Several instances of this kind have come under my notice. They were accompanied with dyspnœa, which was at first paroxysmal as in asthma, afterwards became more constant, and in the end was unremitted and almost insupportable. When a very young practitioner I witnessed the case of a female upon whom a very respectable surgeon of this city operated. The fascia of the pectoral muscle was affected, and the case was pronounced very unpromising. I attended after the operation, and regularly dressed the wound. Good pus was secreted, the cavity gradually filled up, the granulations were healthy, and the prospect was for a time flattering. But by degrees the granulations lost their healthy aspect; dyspnœa came on, at first paroxysmal, afterwards constant; and, for many weeks before her death, the patient, unable to lie in bed, was compelled

to sit supported in a chair. In another similar case I had an opportunity of making a post mortem examination. The lungs were found full of tubercles, which, when opened, presented the striated structure peculiar to cancerous tumours.

The wrinkled state of the skin to which I have alluded as strongly characteristic of scirrhus of the mamma is not always present. I have seen several cases in which the gland, instead of being contracted, was considerably enlarged, the skin being smooth, tense, and glossy, as if stretched by the expansion of the parts beneath it. In these cases the surface of the breast presented a peculiar stellated appearance, being spangled with little red spots, arising from a disordered state of the cutaneous vessels. In one instance of this kind, the patient experienced much less pain than might have been expected. Sometimes instead of this spangled appearance there is a uniform dark lurid colour of the breast, the gland being much enlarged and of a stony hardness.

I have also witnessed another condition of the skin attendant upon scirrhus of the breast, to which my attention was first called by Dr Physick. Upon running the finger over the surface, you perceive that it is granulated; and a sensation is imparted as if the skin were filled with small shot. I believe that Dr Physick considers this as the most dangerous form of the disease. He told me that he had never known an operation to prove ultimately successful; and my experience fully coincides with his. I once operated upon such a case in the Pennsylvania Hospital. The disease was confined altogether to the breast, and as far as could be ascertained by the sight or the touch was entirely removed. The wound healed kindly, and for a short time the patient appeared comfortable: but in a few months the complaint returned, and soon terminated fatally.

In the above account of the symptoms and progress of cancer in the breast, I do not pretend to great minuteness of description. I wish only to call attention to the more prominent and characteristic traits of the disease, by which it may be distinguished from affections which, though sometimes mistaken for it, are in fact much less serious in their nature, and call for a different treatment and prognosis.

I have never met with cancer of the breast at a very early age, and seldom among very old people. In the majority of cases it occurs after the thirtieth year and about the period of the cessation of the catamenia. I have seen it in males as well as females, though very rarely in the former. A case of the kind fell under my

notice in the Pennsylvania Hospital, upon which my friend Dr JOHN RHEA BARTON operated. The disease, which had reached the axilla, returned after the operation, and proved fatal.

Observation has led me to the belief that cancer is a constitutional affection. We often hear it referred to violence done to the part by blows, falls, or other accident; and it is possible that it may be occasionally developed by such means, where a predisposition to it exists in the system: but I doubt whether genuine cancer ever originates purely from external injuries.

As regards the treatment, I have no confidence whatever in the efficacy of any measures, other than a total extirpation of the diseased part. Even the knife can by no means be relied on as an effectual remedy. There can be no doubt that the progress of a scirrhous tumour may be delayed, occasionally even arrested for a considerable length of time, by depletion, low living, and the avoidance of all exciting causes. But when parts have taken on the genuine cancerous action, and have assumed the peculiar cancerous structure, I am convinced that they cannot be restored to a healthy state by any means which are at present at our command. At least it has never fallen to my lot to experience in my own practice, or to witness in that of others, a result so desirable. Cases of cures are occasionally reported in the journals and other medical works; and I am not disposed to call in question the truth or accuracy of the statements, so far as facts are concerned: but considering the comparative rarity of such supposed cures, it is hardly going beyond the bounds of charity to suppose that the judgment of the practitioner may have been mistaken, and affections recorded as cancerous which have partaken of this character only in their alarming appearance, or in the obstinacy with which they had previously resisted all curative measures. Indeed the very account of symptoms which accompanies these statements affords, in some instances, sufficient evidence that such is really the case.

The strong disposition in the constitution of patients affected with cancer to a return of the local disease, even after every vestige of it has been completely removed, renders it a question how far the use of the knife is desirable. When the axilla or the pectoral muscle is involved, the chance of an ultimate cure is, I believe, very slender indeed. The operation may be carefully and skilfully performed, the wound may heal, and the patient may for some time enjoy apparently perfect health: but in the great majority of cases

the disease will sooner or later reappear. I have operated in about twelve cases of this kind; in some have completely dissected out the glands of the axilla, and have even gone round the clavicle, removing as far as was discoverable every suspicious part; yet with one or two exceptions the patients have ultimately fallen victims to the irresistible encroachments of the malady. In one instance, in which, however, the tumour was soft and approached to the nature of fungus hæmatodes, a permanent cure was effected. The patient was the wife of a poor sailor. The axillary glands were extirpated together with the breast, the wound healed without difficulty, and though seven years have elapsed she has remained free from the disease. In another case, upon which I operated about two years since in the Pennsylvania Hospital, there has hitherto been no sign of a return. With these two exceptions, in one of which the nature of the complaint was doubtful, in the other a sufficient period has not yet elapsed to test the success of the operation, I have met with no case of cancer of the breast extending into the axilla which has not terminated in death. It generally makes its reappearance within a year after the operation, sometimes, however, not till after a period of two years. The cicatrix is often the part first affected. It becomes painful, and on examination is found red, elevated, and hardened. There is now, I conceive, no chance of escape. I have repeatedly seen the cicatrix removed by a second operation; but never with final success. The result of my experience in these axillary cases, as well as in those in which the pectoral muscle or its fascia is affected is a conclusion never to advise the employment of the knife, as affording a reasonable chance of permanent advantage. When a patient is brought to me under these circumstances, I feel myself bound to inform her candidly of her situation. I lay fairly before her the very slender prospect of an ultimate cure; and state my opinion, that the most she can reasonably expect from an operation is the delay of one or two years in the progress of her disorder. If she then decide to submit to the knife, I do not feel myself justified in withholding it. I never, however, recommend it.

Even when the gland alone is affected we can by no means be certain that an operation will secure the patient. It is undoubtedly true that females affected with cancerous mamma have been entirely and permanently relieved by the removal of the diseased part. Instances of the kind I have witnessed in my own practice. But there is reason to believe that not a little of the credit which the

knife has acquired in this complaint is properly attributable to the mistaking of tumours comparatively harmless in their nature for genuine scirrhus. That inexperienced practitioners are very liable to such mistakes is evinced I think by the cases which I have reported in a former number of this Journal. In my own practice I have seen enough to be convinced that, though the operation may have been performed under the most favourable circumstances, the disease will frequently return, and march regularly on to a fatal issue. Some years since I operated upon a black woman, whose breast was readily moveable, and her axilla perfectly free from disease. She was soon afterwards again attacked, and died with a most terrific form of cancer. The case of a married lady from the country, in whom also the complaint was confined to the breast, was not less unhappy. The whole mamma was removed, and not a vestige of unhealthy structure remained. The wound healed in the kindest manner, and for a year she enjoyed excellent health. At the end of this time she came to the city to consult me relative to some uneasiness in the cicatrix, which, upon being examined, appeared to be in a suspicious state. I directed leeches and an antiphlogistic course, under which plan there seemed to be some improvement. The complaint, however, soon developed itself. Not only did the cicatrix take on a decided cancerous action, but the other breast became involved, and in less than two years after the operation she expired. Several years ago I removed a scirrhous tumour from the breast of a young woman, allowing the healthy portion of the gland to remain. The complaint returned, and within two years ended in death. Dr Hewson, who examined the body, informed me that the viscera were studded with cancerous tubercles. Notwithstanding these discouraging cases, the instances of success are sufficiently numerous to authorize the surgeon to recommend an operation when the disease has not travelled beyond the organ in which it was originally seated.

When the tumour in the breast is very small, it is a question of some interest, whether the whole gland should be dissected out, or only the diseased portion. If the patient is beyond the meridian of life, and the breast can be of no further use in lactation, I unhesitatingly remove the whole. But in cases of young women, where the nipple is not affected, and the duties of a mother may have to be performed, there is certainly ground for hesitation. Under these circumstances I think it proper to allow the patient her choice. At

the worst, she can only be subjected to a second operation. A young woman came into my care in the Pennsylvania Hospital with a suspicious tumour of the mamma. It was small, and might be so extirpated as to leave a considerable portion of the gland untouched. The state of the case was properly represented to her. She decided that at first the tumour only should be removed, and that, should there be a return of the complaint, she would again submit to the knife. The operation was performed in the manner she desired; and, though several years have since passed, the breast has remained perfectly sound. A case has been related above in which the termination was very different.

Before leaving the subject of cancer of the breast I would observe, that of the cysts which are occasionally formed in the mamma, having the character of hydatids, some are entirely innocent, while others take on the cancerous action and destroy life. A case of the latter kind occurred to me in the year 1819, which I will relate. The patient was an elderly female, and had for twenty years been affected with a tumour of the breast, about the size of an egg, which at the end of this time had disappeared. About fifteen months before I saw her it had returned, and during this period had been gradually increasing. When I examined her, the mamma was more than twice its natural size, presenting to the eye and touch a smooth surface, without pain, or any other mark of inflammation. She was unwilling to submit to an operation, and for several months the case went on with little or no medical treatment. The breast became enormously enlarged—being equal in circumference at its base to an ordinary wash-hand basin, and quite prominent. It was now very painful, and being again requested to examine it, I found by the distinct fluctuation, that it was filled with fluid. I advised that the fluid should be gradually discharged by repeated punctures; but a dread of cutting instruments prevented her from assenting, and she continued to suffer from the pain of increasing distention till the tumour opened. I was informed that about a gallon of dark coloured and offensive fluid was discharged. The breast now shrunk even within its natural dimensions. Afterwards, however, it became enlarged and inflamed, and a cauliflower excrescence shot out of the ulcerated opening. An offensive discharge took place; hectic fever set in; and the complaint assumed so alarming an appearance that her terror of the knife was entirely overcome, and she even requested an operation. As the

axilla was free from disease, and the tumour appeared to have no attachment to the pectoral muscle, I was not without hopes that its removal might afford permanent relief. Upon dissecting out this enormous cyst, contrary to my expectation, I found it adhering by a small point to the fascia of the pectoral muscle. The whole, however, was removed; the wound nearly if not quite healed; every symptom of hectic fever vanished; and the general health seemed quite restored. The disease, however, was afterwards renewed in the breast—and, as in every other case that I have observed, in which the pectoral muscle or its fascia was involved, ceased its ravages only with the death of the patient.

On the whole with regard to cancer I am willing to confess, after considerable observation and experience, that I am much discouraged as to the final success of the operation. I repeat that in cases where the disease has penetrated into the axilla, or has fixed upon the parts beneath the breast, I never resort to the knife unless at the particular request of the patient, with a full knowledge on her part of my own conviction as to the probable inefficiency of the remedy. When the breast alone is affected, I advise the operation: but even here I consider it as affording but a doubtful prospect of escape; and however much I may be grieved, am never disappointed at finding my patient again suffering with the disease and finally sinking under it.

Notwithstanding, however, this discouraging view, when I contemplate the goodness of a bountiful providence, and reflect upon the numerous instances in which remedies have been graciously provided for the ills with which we are afflicted, I cannot but entertain the hope and even the conviction that some mode of cure will yet be discovered for this horrible malady: and the man who may become the instrument of this merciful dispensation of divine favour will obtain and deserve an honour among men not less imperishable than that which encircles the memory of the illustrious JENNER.

ARTICLE IX.—*On the Medical Properties of Iodine. By* JOHN BELL, M.D.

Iodine, first used as a medicine for the cure of goitre, has within these few years past been employed in various other maladies, such as amenorrhœa, gonorrhœa, and scrofula. Like every other article of the materia medica, it has been subjected more to the hazardous trials of empiricism than to the guidance of careful observation and methodical induction: and such must ever continue to be the case so long as physicians in search of its specific effects overlook the necessary modifications which it undergoes according to the condition of the tissue or organ to which it is applied, or on which it more immediately and permanently operates. Dissimilar effects consequent on its administration are continually attributed to the intrinsic properties of a drug in place of the difference of circumstances, that is of the animal economy or particular portions of it, pending the time of its administration. Thus a very brief retrospection of the history of medicine makes us acquainted with the immense amount of suffering and waste of lives caused by the empirical use of mercury and bark. Syphilis in certain stages was arrested by the introduction of mercury into the body of the patient; and it was straightway inferred that the alleged poison of the disease had met with its antidote in the medicine. Did a person, however, become worse under the mercurial treatment, the syphilis was declared to be very obstinate and to require still more mercury. Ulcerated throat, cutaneous disease, pains of the joints, nodes, all were referred to the malady;—and although the truth was finally revealed, and it was discovered that all these disorders could be produced by the various preparations of the metal, yet the partial abandonment of one alleged antidote or specific was only the signal for a search after others, and the mineral acids and the preparations of gold were set forth as the means of accomplishing what mercury had failed to perform. The like pernicious maxims were held in regard to the use of this latter in fevers; and with still more fatal results, because the sphere of mischievous operation was so much more extensive. Nothing benefited by these lessons, the majority of our physicians continue still to indulge in a similar error in the employment of the Peruvian bark. Its efficacy in intermittent fever of a certain grade was regarded as a pledge of its virtues in all the varieties not only of

this fever but in all periodical diseases. Failures and disasters followed of course the practice of this creed; but the evil was not traced to its source. In place of inquiring whether the human body, suffering under periodical fever, was in that state in which no impressions effected by tonic or astringent articles could be tolerated at all with any safety or advantage to the patient, the medical adviser took it for granted that intermission or periodicity was an unit, and the only questions to be asked were the dose of the bark and the time for its administration. Some wholesale reasoners were indeed disinclined to allow of any restrictions; and whether there was apyrexia, or chill, or fever, the bark was to be poured in. When, finally, some glimmerings of light broke in upon these benighted philosophers, and they were compelled to acknowledge that bark was not a specific for intermittent fever,—what course did they pursue? Inquire into the particular state of the stomach of the patient, compare it with that in which the medicine had been serviceable, and endeavour to bring by other measures the former into the same favourable state as the latter? By no means: they began ransacking first the domain of Flora and then the interior of the earth in order to discover other tonics to relieve the debility induced by fever. Arsenic followed bark, to be in its turn succeeded by iron and copper and zinc and mercury; and thus for the sake of preventing the return of a chill the poor patient is to be slowly poisoned, and left a prey to the wasting effects of chronic gastritis, engorged liver, hypochondriasis and all the horrors of dyspepsia. One would have supposed, by the common rules of logic, that if the hope of benefit to a patient be predicated of the known effects of two co-operative causes or agencies, the disappointment must be sought for in that one of these which is most mobile and exposed to continual fluctuations. The medicine being presumed of uniform intrinsic properties, the inquiry into the failure must then of course be sought for in the other cause, viz. the human body in general or the organ in particular to which it was applied, and the mutations in t state of which are productive of the most diversified results even from the contact of the same substance.

Iodine had been administered in full doses by inunction to a large number of persons for a length of time by Dr Coindet and other physicians of Geneva for the cure of goitre before it was prescribed internally in one of the Italian hospitals for amenorrhœa. The first reports of its effects in this last mentioned disease were of a very

encouraging character—menses restored—colour and strength and appetite regained. Not long after this, we received accounts of the injurious and even fatal operation of the medicine; and its use was formally prohibited by a government ever meddling and intrusive, occasionally intent on checking evil, more generally jealous of all improvement. The transition on the part of the medical faculty from exalted eulogy to indiscriminate reprobation, though in character with much of the history of the materia medica, was at direct variance with the principles of physiology. An agent tolerated and even well received by the animal economy at one time cannot by any show of reasoning be proved deleterious to it at all times, and yet this would be the singular syllogism of the prescribers of iodine to whom I now have especial reference.

The first case of *amenorrhœa* in which I employed the medicine was of a female aged thirty years, who had been married and had been pregnant at different times but never had a child at the full term. For three years prior to my visiting her she had suffered from prolapsus uteri and had only menstruated three times during this period. Encouraged by the relief afforded to a neighbour by the introduction of a pessary, she allowed an examination per vaginam in her own case. The pessary was used by her with the effect of affording speedy cure and comfort, and ability to move about and walk with freedom in place of spending, as she had been compelled to do, much of her time in a recumbent posture. The first menstrual discharge was effected three months after the introduction of the pessary, and followed the use of the volatile tincture of guaiacum and balsam copaiba—in the proportion of two parts of the former to one of the latter; dose, a tea spoonful at first three times and afterwards twice a day. This mixture had been taken during the preceding month, and in the last instance for four days before the coming on of the menses. Two months after this she had a menstrual discharge,—having made use for five days before of the mixture as above and an injection per vaginam of aqua ammoniæ in milk three times a day, and pediluvium at bed time. I ought not however to omit mentioning an additional means of exciting the uterus, which was also had recourse to once before the first discharge: it was the introduction of the end of a large gum elastic catheter into the os tincæ, which in this case was wide and the sides soft, full and flaccid, and giving it for some minutes a rotatory movement.

On the following month pills of myrrh, aloes and rhubarb were

prescribed, alternating with the volatile tincture of guaiacum; and the injection per vaginam of aqua ammoniæ xv to xx drops in milk, three times a day for five days, with the pediluvium at night was had recourse to: but all without effect.

Two months after this the tincture of the iodine was prescribed.

The patient's temperament is lymphatic—flesh soft—cellular tissue puffed—complexion pale. She is subject to sick headach—tongue usually moist, at times somewhat loaded—pulse little affected. This state, which seemed to authorize the stimulating emmenagogues already given, would, it was thought, justify the use of the iodine. A saturated tincture of two scruples to an ounce of alcohol was the form prescribed*. Of this twenty drops were given twice a day, and the dose gradually increased to forty. After twelve days use it was discontinued, without any other effect than pain in the back and head, and thirst after the first four days. The use of the tincture was resumed in a fortnight in doses of forty drops twice and after a while thrice a day, and continued for fifteen days; during the latter part of which the ammoniacal injection and introduction of the catheter were practised, but all without exciting any menstrual effort. Aloes, rhubarb, and sulphate of iron, in pills, were directed for the next month, and followed by ergot in scruple doses twice a day, and the ammoniacal injection. The ergot, on account of its causing nausea and vomiting, was discontinued. The tincture of the iodine was, after a few days, directed in its place, in a dose of twenty-five drops, to be gradually increased to fifty, three times a day; and the liquor of ammonia twenty drops, four times a day, mixed with milk, injected per vaginam. The face at this time and the extremities were puffed and swelled. On the second day of this last treatment the patient was seized with a severe attack of spasm of the stomach, followed by pains in the back, and on the fourth the menses appeared, and continued for two or three days to flow very freely.

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* Mr Durand, one of our most intelligent apothecaries, states that an ounce of alcohol, of the common strength, will not take up more than two scruples of the iodine. To this tincture I usually direct the addition of a drachm of spirits of lavender compound, which may also be advantageously combined with the solution of the hydriodate of potass. The proportions of which I generally make use are one drachm of the salt to an ounce of water. Where there is any doubt respecting the ability of the stomach to tolerate the medicine, it is safer to begin with either of these preparations in doses of ten drops twice a day.

Three weeks from this time the same measures, by the use as above of the tincture of iodine and the injection, were had recourse to, but without effect. On the following month the same means were equally unproductive of any perceptible results. The use of the sulphate of iron and rhubarb, in pills, for two weeks in the next month, followed by the volatile tincture of guaiacum in doses of a tea spoonful twice a day for two days, produced a menstrual discharge for twenty-four hours. No such effect, however, occurred from the use of the guaiacum tincture given three weeks afterwards, although the pills of sulphate of iron and rhubarb had been taken as before.

No emmenagogue stimulus was given for a period of two months. The first prescribed was a solution of a salt of the iodine, the hydriodate of potass, ʒi. to ℥i. of water. Dose, thirty drops, twice a day, gradually increased to forty, for ten days. The ammoniacal injection was resorted to also, but no uterine secretion was effected. Six days use of the ergot, and canella alba, aa. ℈i. b. d. three months from this time was followed by a menstrual discharge for a day and a night. The like effect succeeded the use of the turpentine during the last spring. The subsequent exhibition of this latter medicine even in larger doses, and for a longer period than that recommended as so successful by some of the late European continental physicians, has not in this case been attended with any beneficial results.

The emmenagogue powers of the iodine have not, from the preceding account, any stronger claims on our confidence than the tincture of guaiacum, the ergot, or the turpentine. As it is only by means of direct comparison that we can duly appreciate the value of a medicine, we have to regret that many of the boasted emmenagogues have not been more fully tried in this way before they were set forth by physicians in terms of such extravagant and idle eulogy. My patient menstruated six times during two years;—a result certainly which affords me no cause of triumph, but which will, I believe, be found as successful as the average relief obtained in such cases. The continued use of the pessary enables her to avoid a train of painful feelings, and to enjoy comparatively good health. Her sedentary habits forbid me perhaps to hope for more.

In the case of a female with chronic gastro-enteritis, and a continued morbid state of the heart—seemingly organic disease of the part, and acute headach, of a year's duration, copious and frequent venesection having been frequently used, without however remov-

ing the hardness and frequency of the pulse, the tincture of iodine, as above, was given in doses of twenty drops, twice a day, as an emmenagogue. The medicine was continued for eight days, the dose being gradually increased to thirty drops, but without any beneficial result. A return of irritation of the stomach and some pain in the bowels was the signal for discontinuing the tincture. The associated use of the injection per vaginam of the aqua ammoniæ in milk was of no avail.

In a case of amenorrhœa of three months duration, the tincture of the iodine restored in a short time the menstrual discharge, and freed the patient from fluor albus to which she had been much subjected. Since then for a period of two years she has been quite regular, and free from the pains under which she formerly suffered at the menstrual epoch.

Another female similarly circumstanced used a solution of the hydriodate of potass with equal benefit.

A young woman who had ceased to menstruate for four or five months had the secretion restored after a few days use of the solution as above.

The medicine was given to two young females in the Philadelphia Alms House, in both of whom it brought back so soon the irritable and pained state of the stomach of which they had so often before complained in connexion with their amenorrhœa of long standing as to require of me to desist from prescribing it any longer to them. These patients were doubtless similar to the Italian ones, on whom the iodine was said to have produced such fatal effects as to lead to the entire disuse of the medicine:—they were prone to acute inflammatory attacks and pain of the head, chest and stomach, with great morbid activity of the heart and arteries. My justification for prescribing the iodine to them was the free and frequent sanguineous depletion to which they had been before subjected, and the then comparatively good state of their stomach.

In *leucorrhœa* I have directed the tincture of the iodine in some cases and the solution of the hydriodate of potass in others with speedy and permanently beneficial effects.

In *gonorrhœa* the medicine has at times a very happy operation—soon arresting the discharge. It was the only thing by which I was enabled to cure a most protracted and virulent case of this disease in a young female, accompanied with ulceration of the throat and

skin. She had while taking the iodine two returns of very copious menstrual discharge.

Another female advanced in life who received the gonorrhœal infection from a drunken husband, and who had in consequence copious discharge with soreness of the labiæ, was cured in ten days by the solution of the hydriodate of potass ʒi. to ʒi. of water with a little spt. lav. comp. She was relieved at the same time from a distressing pain in the back under which she had long suffered and from an herpetic eruption on her elbows of thirty years duration.

A man who had gonorrhœa, phymosis, ulcers of the prepuce and swelled testicle, was cured by the use of the tincture of iodine in doses of twenty gradually increased to forty drops twice a day; and the application of lunar caustic to the sores. The hardness and swelling of the testicle had entirely disappeared. These salutary effects were brought about in a period of three weeks. After a lapse of twenty days however, this person presented himself to me with a return of his urethral discharge. A solution of the hydriodate of potass as above was prescribed in doses of thirty gradually increased to forty drops twice a day, in place of the tincture, the taste of which was to him so nauseating: an injection of sulphate of copper and opium in water was also directed. This treatment was persevered in for ten days without success. The balsam of copaiba with the cubebs and spirits of nitre in a gum mixture were then prescribed; and in forty-eight hours the running was completely stopped. Warty excrescences on the prepuce were of much more troublesome management, and demanded longer time for their removal than the urethral discharge.

The iodine in some cases of gonorrhœa would seem to merit as favourable a mention as the most approved remedies for this disease; to which in others again it is of manifestly inferior power. It may be received as one of the varieties to which practitioners must often find it necessary to have recourse in this disease, especially if it have been at all protracted before they are consulted on the subject.

In ulcers of the mouth and throat, whether consecutive on syphilis, or proceeding from other causes, I have derived the very best effects from the iodine. Its operation on the mucous surface of the urethra and vagina induced me to make trial of it in ulcers of other parts of the mucous system; nor have I been disappointed in my anticipations. It has been especially serviceable in females of scrophulous habits, in whom troublesome aphthæ and ulcers of

the mouth and velum palati appeared contemporaneously with leucorrhœa, and for which mercury is not unfrequently prescribed with very hurtful effects.

I have found the iodine in tincture, or solution of the salt, display very satisfactory curative powers in *scrofulous* ulcers of the neck; and I have frequently prescribed it in cases of *venereal bubo* with apparently the effect of discussing it, or if too far advanced, and that it was opened, of accelerating granulation and cicatrization.

In a case of *enlarged* and *indurated testicle* of long standing, the patient in obedience to my directions made use of the ointment of hydriodate of potass—composed of a scruple of the salt and an ounce of lard—externally, and took internally first the tincture then the solution as above for a month, without any notable change being effected. Venesection from the arm and leeching the scrotum, and fomentations had been resorted to before beginning with the iodine.

I have at this time a patient who is using the medicine both internally and externally for a disease of the hip joint accompanied with a tense tumefaction of the outer portion of the thigh; and pain of the knee, especially at night. Most of the remedies recommended in such cases have been employed in this except moxa and the seton. Cupping very frequently over the hip, and a discharge kept up for a long time from the vicinity of the joint by tartar emetic ointment, and blistering have only afforded partial relief. After a fortnight's use of the iodine the swelling immediately over the joint had nearly disappeared, and allowed me for the first time in many months to feel it quite distinctly. For fear any ignorant philanthropist on seeing these pages should regret that my poor patient had not been recommended to use the popular panacea, I will take this opportunity of saying that he had tried some bottles of it prior to his coming under my care without any benefit whatever.

The apparent impunity with which the animal economy can tolerate large doses of the iodine must not be received as evidence either of its inertness or of the uniform safety of its use. Whatever agent operates so decidedly on the nutritive organs by causing a diminution and removal of morbid growths, as in goitre and scrofulous tumours, or in excessive obesity, must, we may be assured, have a strong and at times paramount power of inflicting great injury. Many mineral and vegetable poisons can, we know, be often tolerated to an extent in some cases which would prove speedily fatal in others. Iodine in small doses will, like many of these articles, exert a tonic

power and create appetite, precisely in virtue of its direct effects on organic nutrition, and tendency to make absorption or waste predominate over secretion or supply. This state of the body is almost always accompanied by a craving for food and an energetic action of the digestive passages, as we find in marasmus, except where there is phlogosis of the gastric surface. We shall often therefore be led into very serious errors if we mistake a craving appetite, whether occurring in chronic disease or as a consequence of our remedies, for evidence of the wants of healthful nutrition, or a sign of returning health and strength.

When the tongue is of a bright red at its end and borders, or is very dry and furred, and the stomach hot and intolerant of pressure, or if there be pain or disorder of the bowels, the iodine ought not to be given. In an opposite state of the intestinal canal, with rather a sluggish circulation and lymphatic temperament, this medicine will be found to display effects of unequivocal power and benefit.

Analytical Reviews.

ARTICLE X.—*Traité sur les Gastralgies et les Entéralgies, ou Maladies Nerveuses de l'Estomac et des Intestins. Par* J. P. T. BARRAS, *Docteur en Médecine de la Faculté de Paris, &c. &c. Seconde Edition, revue, corrigée, et augmentée. Paris. Oct.* 1827. *Pp.* 386.

Quandò talis amentia et abusus cessaturus est? et quandò aurea mediocritas et sobrietas in territorio medicorum triumphatura?—*Schmidtmann.*

HAVING in several of the previous numbers of this Journal attempted to introduce our readers to an acquaintance with the prominent doctrines of the physiological school of medicine; we conceive it to be equally our duty, as Editors, to lay before them also, as opportunities offer, some of the objections to which these doctrines are supposed to be liable, and the different explanations given by their opponents of the same morbid phenomena. To this intent, we have made choice, on the present occasion, of the work of Dr BARRAS, the title of which is above recited.

Every one in the least degree acquainted with the writings of BROUSSAIS, is aware of the important rank he assigns to lesions of the mucous membrane of the stomach and bowels, but especially of the former, in the production of many of those groups of symptoms, which had been erected by the nosologists into separate and distinct diseases. To demonstrate the incorrectness of this pathology, in reference to various morbid phenomena, and to trace these to their true cause, is the avowed object of the publication before us.

The author, it appears, had laboured for a number of years under a gastric affection; which, he informs us, was invariably aggravated when treated according to the principles of the physiological doctrine, but had its symptoms greatly ameliorated, and was finally

cured, as soon as such treatment was abandoned, and one of an opposite character adopted.

These circumstances very naturally led him to a more particular investigation of the several morbid conditions of the digestive organs, the result of which has enabled him, he conceives, to demonstrate that many of those affections referred, by BROUSSAIS and his disciples, invariably to an inflammation, for the most part chronic, of the gastro-intestinal mucous membrane, are in fact pure nervous diseases, that is to say, "lesions of sensibility, without inflammation, or alterations of structure;"—diseases which are always kept up and increased by the remedies adapted to a case of gastro-enteritis.

"We are far, however, from asserting," remarks Dr BARRAS, "that in these cases the nervous tissue is in its normal condition; the disorder of its properties and of its functions announces, on the contrary, that it has experienced a morbid change; but this change is not an organic alteration, nor an inflammation, for our senses cannot detect it." Page 13, *note*.

Dr B. has designated the diseases here referred to by the terms Gastralgia and Enteralgia; uniting under these two general denominations all the gastro-intestinal neuroses; adding the word hypochondriac to indicate those cases which, he observes, are of frequent occurrence, where "a nervous affection of the encephalon is added to that of the primæ viæ."

The nervous affections of the stomach and bowels being supposed seldom or ever to exist separately, the term gastralgia, gastric neurosis, or nervous disease of the stomach, is alone employed in the course of the present work;—the existence of the intestinal disease being however always understood.

It would not accord with our limits to enter into a examination of the long, but very imperfect detail which Dr B. has presented to us of the commencement, progress and termination of his own case. From this detail, however, it is very evident that the disease under which he laboured, whatever we may suppose to have been its real character, was at no period, during its whole course, submitted to any regular or consistent plan of treatment, and that its symptoms and progress by no means warrant the positive conclusions drawn from them by the author. The same observations are, in a measure, applicable to nearly the whole of the thirty-three additional cases related in the work under review. Nevertheless, it is almost solely upon the phenomena of these cases that the pathology of the author in relation to the gastralgia is founded:—speaking of them in his first chapter, he remarks:—

"These facts prove that the physiological physicians are in error in regard to the nature of gastric affections, and that upon this subject, as upon many others, far from having aided the progress of medicine, they have done it a serious injury, in turning it aside from the certain road of observation, to bring it back to the dangerous field of speculation (*systêmes*). In a word, I believe I shall be able to demonstrate by conclusive facts, and plausible arguments, 1. That gastro-enteralgia differs essentially from chronic gastro-enteritis. 2. That the theory in which these two diseases are regarded as identical and requiring the same method of treatment, leads to the commission of very serious errors in practice. And 3. That consequently such a theory is a dangerous weapon in the hands of inexperienced practitioners, or of those seduced by the innovations of the new school."

The subject thus stated is confessedly one of the first importance. If the author has been able to demonstrate that certain symptoms referable to the stomach, are entirely unconnected with inflammation, in any grade, of that organ :—if he has succeeded in drawing a distinct line of demarcation between those gastric affections which demand for their removal an antiphlogistic treatment, and those, on the contrary, in which such a course is decidedly prejudicial, he has conferred an invaluable benefit upon medical science, and will be the means, certainly, of preserving the health and lives of innumerable individuals.

It is proper to remark that Dr B. admits the nervous, like the other tissues, to be susceptible of a state of actual inflammation, and of experiencing organic degenerations.

"We cannot deny," he remarks, "to encephalitis, to mielitis, and to neuritis the right of being ranked among the inflammations: but those diseases which consist only in an irritation, atony or mobility of the nervous system, without appreciable lesion of the parts in which they are seated; that is to say, the nervous affections, which the ancients regarded as a morbid condition without matter, *intemperiem sine materiâ*, and which the moderns, before the period of the physiological doctrine, recognized under the appellation of neuroses; cannot be placed in the same class with the former."

He is willing to confess, also, that most of the symptoms attendant upon nervous diseases are frequently symptomatic of, or in other words, result from, inflammation.

"We are now aware," says he, "that the greater part of those symptoms which characterize the diseases termed nervous, may depend upon an acute or chronic phlegmasia, upon an organic lesion, or upon a foreign body in the interior of our organs: we know, for example, that convulsions and delirium result, frequently, from the presence of worms in the intestines, from a scirrhus of the brain, or ossification of its meninges; from an arachnitis, an encephalitis, or in very irritable constitutions, from any other inflammation. We therefore infer that the essential neuroses are of less frequent occurrence than was formerly believed, and that the diseases regarded as such, for the last forty years, are in fact genuine

chronic (*latentes*) inflammations; morbid affections then but little understood, and for having described which, particularly as they occur in the mucous membrane of the digestive canal, with much greater accuracy than they had been before the appearance of his *Histoire des Phlegmasies Chroniques*, M. Broussais, preceded in this, however, by M. Pujol, has deserved highly of the profession."

But because many nervous phenomena depend frequently upon a phlegmasia, it does not follow, Dr B. conceives, as a necessary consequence, that the neuroses are to be considered invariably as inflammations; on the contrary, he is persuaded that idiopathic neuroses, that is, nervous diseases independent of inflammation, or any other organic affection, are still of sufficiently frequent occurrence to demand a particular attention.

"Consult," he remarks, "the treatises upon mental alienation, which is the nervous disease that furnishes the most frequent opportunities for examinations after death, and we shall find that the cases in which alterations of tissue are discoverable are the fewest in number. Even when alterations are found to exist, they are frequently of too slight a character to permit us to attribute to them the symptoms by which the disease had been characterized:—they are then to be considered as complications, or concomitant lesions; more especially as it is not uncommon to discover disorganizations to a much greater extent, without the subjects in whom they occur having been insane.

"A very little reflection upon the diseases of the nerves is sufficient to convince us, that the lesion of tissue in which they consist, is frequently inappreciable. The fact is, if these diseases depended always upon either an organic affection or upon a chronic (*latente*) inflammation of the encephalon or of its meninges, of the stomach, or any other portion of the system, they would never be recovered from in the first case, and but rarely in the second; but the greater number of the neuroses, on the contrary, disappear entirely; or rather for a time, for we are aware how liable they are to relapses, especially the vesaniæ."

In a work like the present, particularly when we consider the important task the author has proposed to accomplish by its publication, it was reasonable to expect that we should have met with a somewhat minute investigation of the physiology and pathology of the nervous system, or at least, some general views in relation to both. But, so far as it relates to the physiology of the nerves, scarcely a word is said in the whole volume, and the section upon the pathology of the neuroses (*Théories des Névroses*) contains nothing but a few naked propositions and general statements, without any attempt being made to establish their correctness by facts or argument. It is true, no doubt, that the author considered the propositions which he has here laid down, to be necessary corollaries from the facts which are supposed to have been developed by the cases previously detailed, and the remarks appended to each. Let this be

as it may, we nevertheless conceive it a duty, which he owed as well to himself as to his readers, to have bestowed upon this portion of his subject a greater degree of labour; to have presented, with more of development, his peculiar views of the nature and varieties of nervous diseases, and to have compared these views with the several facts, as well physiological as pathological, by which they are supposed to be established.

The following, indeed, constitutes nearly all that the author has given to us in relation to the pathology of what he terms the purely nervous affections.

"Every thing proves that there exist neuroses from irritation, or excess of nervous *tonicity*, and neuroses from weakness, or deficiency of nervous tone; the same as there occur sthenic and asthenic inflammations, active and passive hemorrhages, &c. The first of these neuroses, we denominate neuroses from *erethism*, and the second neuroses from *atony*. Tetanus is the prototype of the former, and paralysis of the latter. Such are the two principal elements which constitute the nervous diseases. The one is characterised by an *exaltation of sensibility*, and a derangement of the functions of the nervous system; the other, by a *diminution of sensibility*, with disorder of the nervous functions.

"But as nature always proceeds by insensible degrees, and never by leaps or bounds, there is found, between the most decided cases of erethism and of atony, a variety of shades more or less approaching to irritation or to debility,—so true is it, that extremes meet. In many instances it even becomes difficult, or very nearly impossible, to determine whether the energy of the nerves and brain is augmented or diminished. The disease appears then only to consist in an extraordinary *mobility* of the organs of sensation, and has, for its principal symptom, merely an aberration of the sensibility and functions of those organs. There is no doubt but that, in all such cases, there is an increase or deficiency of nervous power; but so little marked is the character of the existing affection, that frequently we can only discover its real nature from the preceding symptoms, and from the effects of remedies prescribed by way of experiment.

"The diagnosis of the nervous diseases presents another difficulty, which is, that although the morbid condition of the nerves termed erethism, or that denominated atony, may continue to exist during the whole course of the same neurosis, yet, it is frequently the case, that these pathological modifications succeed to, or replace each other, alternately; so that many of the neuroses, when of any continuance, present, in the phenomena by which they are characterised, a succession of excitation and of weakness."

The terms erethism and atony by which the author has designated the two opposite conditions of the nerves, upon which all purely nervous affections are supposed to depend, he has adopted, he informs us, merely from their having been already in use, and because no better ones occurred to him; he confesses, however, that neither of them conveys a correct idea of what it is intended to represent.

"What have been denominated erethism and atony of the nervous system," he remarks, "consist in two particular modifications of the nerves, unknown in their essence, and of which it is difficult to form a conception *unless we have ourselves experienced them.*"

The term atony, in particular, he has pronounced altogether improper, at least, in the greater number of the cases to which it is applied:—the nerves being never in a state of atony, strictly speaking, excepting when they have lost their sensibility, as in anesthesia and paralysis.

"The morbid condition of the nerves occurring in hypochondriasis, melancholia, &c. which is expressed by the term nervous atony, consists rather in a species of feebleness; for it is aggravated by debilitating remedies, and cured by tonics; but to this feebleness is frequently joined a very great irritability, that is to say, an *excessive sensibility*, an acute susceptibility, which exists principally after the abuse of antiphlogistics, and which should not be confounded with true nervous erethism.

"The character of the neuroses, whether of irritation or of debility, results from a difference in their causes, and in the temperaments, and individual peculiarities of the patients. A too high degree of atmospheric temperature, for example, irritates the nerves, while a humid state of the air weakens them:—individuals of a strong constitution will be affected, in general, with neuroses from erethism, while those who have a weak and delicate constitution will experience, more commonly, neuroses from atony. But when once they have occurred, these two morbid conditions may, reciprocally, produce each other; so that their alternate succession has in it nothing surprising."

On many points connected with the pathology of the nervous system, we confess, in common with the majority of the profession, our almost total ignorance; and this ignorance, we cannot say, has been in any degree removed by the labours of Dr Barras.

It is true, when we are told that nervous affections are of two varieties, the one accompanied with an increase, the other with a diminution of sensibility,—the first depending upon an irritation or excess of tonicity of the nervous system, the second upon a diminution of this tone,—there is something definite and plausible in the statement, which gives to it the appearance of truth: but when the author informs us, that in many cases of the neuroses, it is difficult to determine whether the energy of the nerves and brain is diminished or augmented,—whether there be excess or deficiency of sensibility, and that, frequently, this can only be determined by commemorative symptoms, and by attending to the effects produced by remedies administered by way of experiment, (*qu'à l'aide des tâtonnemens thérapeutiques*) we must say, that although this may possibly be the statement of a fact, it is one of which we can form no conception. But the difficulty, according to Dr B., does not stop

here; for notwithstanding he has set down an exaltation of sensibility as the characteristic of the neuroses from excess of tonicity, and a diminution (*affaiblissement*) of sensibility as characterising those from deficiency of nervous tone; yet we are afterwards told, that these latter are, likewise, frequently accompanied by excessive sensibility.—It was the duty of our author, to have stated explicitly, if it were in his power, in what the *excessive sensibility* of the atonic neuroses differs from the same phenomenon when it accompanies those from erethism, and by what means we are to avoid confounding the one with the other.

The ideas of our author in relation to the pathology of the neuroses appear to us, in fact, to be extremely confused and little calculated to lead to accurate practical results; and this we apprehend is to be attributed to his not having, in the study of these affections, paid sufficient attention to the physiology of the nervous system. He has hinted at the important facts which have been added to this part of physiology by the labours of MM. Gall, Magendie, Ch. Bell, Laurencet and Foville, but we do not perceive, that the discoveries of these gentlemen have had any beneficial effect upon the opinions which he has emitted.

He has not even attempted to prove the proposition upon which the whole of his pathology of the gastralgia depends, viz. that disease may be confined strictly to the nervous tissue of a part or organ. Now we believe that this will be found seldom, if ever, to be the case; with Dr Boisseau we are of opinion, that to locate diseases in either the minute ramifications of nerves, the sanguineous, or the lymphatic capillaries which enter into the structure of an organ, is a mere pathological subtilty. We cannot conceive that one of these parts can be in a morbid state without the disease being quickly extended to the others.

What our author has termed *erethism*, that is irritation of the nerves*, would appear from his own statements to be a condition closely allied to inflammation; thus in his fifth chapter,—

"The love of truth," he remarks, "obliges me to make a concession to the physiological physicians:—it being very certain, as we have more than once acknowledged, that neuralgic pains may be accompanied with inflammation; and the new theory of chronic intestinal diseases would not have merited all the reproaches which are now very properly brought against it, had it been applied to such cases only; but the great inconvenience and even danger of medical sys-

* Erethism, from Ερεθισμα, an irritation,—strictly speaking, *an inflammation*.

tems arise from the desire to adapt them to every fact, while they apply only to a few.

"The association referred to between certain cases of neuralgia and inflammation, takes place, without doubt, in virtue of the axiom '*ubi dolor, ibi affluxus*,' and probably, also, in consequence of the administration of stimulants as remedies in the neuralgia."

After instancing a few examples of this complication, as it occurs on the exterior of the body, the author observes, that the same thing takes place, frequently, in the stomach. It is even probable, he thinks, that the inflammatory neuroses of this organ were of very frequent occurrence formerly, when stimulants were too generally resorted to in the treatment of its chronic affections.

"It is certain however," he adds, "that they are, at present, less liable to be produced by such remedies; thanks to physiological medicine, which, in place of stopping at the proper point in reference to this part of therapeutics, has gone over to the opposite extreme. We nevertheless still meet, in practice, with gastro-enteralgia of an inflammatory character, and this is not astonishing, inasmuch as certain neuroses of the digestive canal approach very nearly to inflammation of its mucous membrane, and may excite the latter without the aid of stimulating remedies. We know that these gastralgias are those from erethism,—those characterized by violent pain,—in a word, the neuralgia of the stomach and intestines.

"With respect to the gastric neuroses of an indolent character, or those attended with little pain, and in which there is only a mobility, or atony of the nerves of the primæ viæ, they are, perhaps, never accompanied with gastro-enteritis, excepting when the latter has been developed by some irritating cause, such as an immoderate employment of stimulants and cordials, of which the patients frequently make use under an idea of strengthening their stomachs; or of drastic purgatives, &c."

In the foregoing instances the gastric inflammation is supposed to be superadded to the neuralgia; but according to Dr B. cases occur in which the inflammation is the primary affection, and the nervous exaltation is only secondary. But the disease is then, he conceives, to be removed from the class of neuroses to that of inflammations, it being a gastritis complicated with nervous symptoms. This complication, we are informed, is more generally observed in irritable subjects;—hypochondriacs and hysterical females. Habitually too great, the sensibility of the stomach and intestines of such individuals passes readily to that degree which constitutes gastro-enteralgia, especially when they are excited by an attack of inflammation.

"For every focus of irritation," he observes, "and consequently of inflammation, occurring in individuals possessed of great irritability, excites in the nerves, in the neighbourhood of the irritating cause, a state of erethism and an exaltation of sensibility which may be propagated to the whole nervous system. We

are not, however, to forget that this state of erethism and exaltation constitutes frequently the primary disease, and that it differs essentially from inflammation, notwithstanding the one may produce the other, and notwithstanding they are many times found united."

This complication of gastro-neuralgia with inflammation explains, according to our author, the real nature of those cases, in which it has been supposed that while one part of the stomach is in a state of *sur-irritation*, other portions of it are, at the same time, labouring under *ab-irritation*, or debility. The treatment of which cases has been considered highly embarrassing, in consequence of the remedies adapted to the existing inflammation augmenting the debility; while the tonics which are subsequently resorted to, with the view of removing the latter, seldom fail to revive the irritation.

Admitting the existence of pure gastric neuroses, such as the author has described; to be able to distinguish them from chronic gastro-enteritis becomes a subject of the very first importance. This distinction Dr B. has attempted to point out, by an examination of the causes of both diseases, and by contrasting their symptoms. Without following him in the detail into which he has entered, it will be sufficient for our present purpose to present a brief summary of the principal circumstances under each of these heads.

The most frequent causes of chronic inflammation of the mucous membrane of the digestive organs are, an acute inflammation of that membrane;—the abuse of alcoholic liquors, or of irritating remedies—emetics, purgatives, &c.; acrid, irritating and corrosive poisons; the presence of a foreign body in the stomach or bowels; the use of too stimulating food; irregularities of diet and all excesses of the table; ice or cold water, taken while the body is in a state of perspiration; a very cold temperature to which the individual is not accustomed; the sanguine temperament; a particular susceptibility to inflammatory affections; the suppression of habitual hemorrhages, or of the perspiration; the repercussion of a cutaneous disease; metastasis of rheumatism or gout; stimulants applied to the skin, affecting sympathetically the primæ viæ; contusions or any other species of violence inflicted upon the epigastric or hypochondriac regions.

On the contrary, the causes of the gastric neuroses are, an hereditary predisposition; a nervous, irritable and delicate temperament; a peculiar irritability of the digestive organs, either hereditary or acquired; the influence of the imagination, and particularly *the fear of being affected with gastro-enteritis, or an organic lesion of*

the stomach; the sympathy in consequence of which the disease of one individual is excited in another; singular antipathies; a sea voyage, when the individual has never, or seldom been at sea; mental emotions, but particularly chagrin, jealousy, disappointment and paroxysms of anger; onanism, excess in venereal enjoyments, and generally, every ungoverned passion; a sedentary and studious life, profound meditation, or close application of the mind to any object, particularly immediately after meals; great heat, or a very humid state of the atmosphere; the winds of the south and west; sudden variations of temperature; an atmosphere charged with electricity; the abuse of blood-letting in the treatment of various diseases, but especially in those of the stomach and bowels; copious hemorrhages; fasting or abstinence; the too long continued use of atonic aliments, of fish, the farinacea; aqueous, diluent and mucilaginous drinks; excessive salivation; lactation; leucorrhœa; chlorosis; a state of pregnancy; the habit, in particular professions, as in that of the tailor, of bending the chest upon the abdomen, in consequence of which the epigastrium is constantly pressed inwards; the immoderate use of white wines, of coffee, tea, and other aromatics; in a word, every thing that has a tendency, either directly, or indirectly, to exalt the nervous sensibility of the stomach and intestines, and by that means to augment the susceptibility of these organs.

Passing from the causes to the *symptoms* of gastro-enteritis and of gastro-enteralgia, we find the leading phenomena of the former to be,—

Pain of the stomach, seldom intense, but constant, and always exasperated by pressure upon the epigastrium; tongue contracted, red at the edges, and furred in the centre; mouth dry and bitter; thirst; want of appetite, even disgust for food; digestion always incomplete; vomiting of the aliment, particularly of solids; in many patients acid and corrosive, or fœtid and rancid eructations; diarrhœa frequent, particularly when the inflammation has extended to the intestines; hectic fever; loss of strength with rapid emaciation, the effect of the disease itself; complexion entirely changed; progress of the complaint uniform; no intermission of the symptoms; prognostic unfavourable,—the greater number of cases terminating fatally, notwithstanding the most rational plan of treatment.

Contrast with the foregoing the principal symptoms of gastralgia, or the neuroses of the primæ viæ; which are, pain in the stomach,

frequently intense, not constant, but remittent, or even intermittent; cessation of the pain upon strong pressure being applied to the abdomen*; sensations of a strange or uncommon character within the stomach; singular pulsations felt at the hypochondriac or epigastric regions†; tongue white and expanded; mouth moist; no thirst; more or less appetite, but capricious or depraved; digestion perfect, and, in many cases, performed with facility; very rarely vomiting of solid food, but very frequently vomiting of liquids and of a glairy matter; eructations insipid and inodorous; obstinate constipation; urine clear, passed frequently, and with a sense of burning; ordinarily no fever; many times, however, febrile movements of short continuance,—very rarely slow and continued fever; preservation of strength, and very little emaciation, provided the patient has not been subjected to too severe a regimen, or to evacuations of blood; complexion natural, or only momentarily altered or from the co-existence of some other disease; frequently cessation of all the symptoms for days or weeks; relapses easily excited; in many cases hypochondriasis, or an affection of the mind, carried to its highest de-

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* "A stomach which has been repeatedly the seat of very intense neuralgic pains, and which is still fatigued by a difficulty of digestion, flatulent distentions, &c. may preserve a species of sensibility, which is augmented by pressure; this I have myself observed; but the attentive physician will not confound such augmentation of sensibility with pain from inflammation." Page 181, *note*.

† These pulsations are sensible to the hand, sometimes even to the eye, and may lead to the supposition that there exists an aneurism of the abdominal aorta or cœliac trunk; especially if there is present at the same time a tumour formed by fæces or gas collected in the transverse arch of the colon. LAENNEC has himself, in conjunction with M. BAYLE, been led into a similar mistake. SCHMIDTMANN refers the pulsations alluded to, to a spasmodic oscillation of the muscular fibres of the stomach and intestines; he thinks they may be distinguished from those of an aneurism, by their not being, like the latter, isochronous with the pulse. According to ALLAN BURNS they are occasioned by nervous contractions of the diaphragm. Dr BARRAS admits that this may be true in certain cases, but with various authorities, he conceives that the spasm which gives rise to them is frequently seated in the great arterial trunks of the abdomen; in many cases, he states, they are isochronous with the pulse, and in hypochondriacs, and hysterical females, the carotids and other exterior arteries present the same phenomenon. According to ALBERS the nervous pulsations are to be distinguished by their sudden appearance,—their being more violent at first, and by their diminishing in intensity after a certain period. They may be produced, also, says Dr B., by encisted tumors of the abdomen, organic lesions of the pancreas, mesentery, &c. Page 183, *et seq*.

gree; prognostic always favourable, and a cure certain, at least under an appropriate treatment.

Having considered the *etiology* and *diagnosis* of the gastro-enteralgia in general, Dr B. proceeds next to describe the individual affections which he includes under this class of diseases. Of these we can only afford room for a very brief notice.

Dyspepsia. Exaltation of the sensibility of the digestive organs, without pain, constituting the first stage of many of the gastro-intestinal neuroses. Appetite variable; most generally digestion slow and difficult, accompanied with a sense of weight, uneasiness or anxiety at the epigastrium, with eructations, borborygma, flatulence, and followed by obstinate constipation. Occasionally, the above are the only morbid phenomena which are observed for a considerable time; the health of the patient being, in other respects, unaffected; most commonly, however, the affection of the stomach becoming painful, passes to the state of cardialgia or gastrodynia. In the majority of cases, the disease is not confined to the stomach, but, more or less promptly, is propagated to other parts of the system, producing there various phenomena. When this transmission has taken place, the augmentation of nervous irritability, mobility or susceptibility, becoming general, the mind of the patient participates in the disease and all the symptoms of hypochondriasis are found successively to appear. Hypochondriasis, Dr B. conceives never to exist without an affection of the encephalon. It commences, however, very frequently, in the stomach, the affection of the brain being secondary. But whenever a cerebral neurosis has become joined to that of the stomach, it reacts upon the latter, and contributes to its continuance and augmentation. Those who consider hypochondriasis as invariably a primary affection either of the digestive organs, or of the encephalon, are both equally in error. Dr B. believes that he can even go so far as to maintain that the primary seat of this vesania is occasionally in neither the stomach nor brain, but that it is sufficient for its production, in individuals predisposed to it, that the nerves of any part of the body be affected. He has seen hypochondriac symptoms occur during external neuralgia, and during pregnancy, and disappear with the removal of the neuralgia, and after delivery.

Cardialgia, gastrodynia, cramp of the stomach, pyrosis. These terms are nearly synonymous; they alike indicate nervous pains of the principal organ of digestion. They are subject to innumerable

varieties, in relation to their intensity, the nature of the sufferings produced by them, and the periods of their recurrence. They sooner or later, if allowed to continue, produce symptoms of hypochondriasis. The pains of the stomach that so frequently accompany *fluor albus*, and which the females who experience them designate, commonly, by the expression "dragging at the stomach," constitute a symptomatic gastralgia, depending evidently upon the empire which the genital organs exercise over the stomach. That these pains are an effect of the leucorrhœa is evident, from their commencing with the latter disease, or soon after it, and disappearing when it is arrested: besides, this gastralgia, which should be named *leucorrhœal*, is ordinarily simple, that is, it is less frequently complicated with hypochondriasis than the generality of the idiopathic neuroses of the alimentary canal.

Enteralgia. Independent of the nervous affections common to the stomach and intestines, the latter may be the seat of particular neuroses, by which the former is not at all affected. Among these is the *nervous*, or *spasmodic colic*, characterised by a sense of twisting, principally around the umbilicus, or in the course of the colon, with an acute pain which is rather diminished than augmented by pressure. The pain frequently terminates within twelve or twenty-four hours, never again to return, while in other instances, like gastralgia, it is reproduced after every meal. This species of colic is unaccompanied with fever, or any symptoms of inflammation. There can be no doubt, the author, however, conceives, that nervous colic will produce *enteritis* or *peritonitis*, if it endure for a long time, is very violent, or occurs in individuals predisposed to inflammation; and hence the necessity of promptly combating it by the administration of soothing and anodyne remedies,—the only means capable of arresting its progress without contributing to the development of inflammation. Like gastralgia, enteralgia may exist under a chronic form and continue for years. Certain cases of enteralgia are accompanied by a discharge from the bowels of a watery or glareous fluid; this the author supposes to be occasioned, like similar discharges which sometimes occur in gastralgia, in consequence of the morbid state of the intestinal nerves occasioning a vicious secretion from the mucous membrane of the bowels, and also, from the difficulty there is experienced, under such circumstances, in the digestion of fluids.

Under the head of nervous colic, Dr B. includes that produced by the immoderate use of cider, beer, &c. and colica pictonum.

Nervous, or *Spasmodic Vomiting*. This frequently takes place independent of any other disease, constituting an essential or idiopathic neurosis of the stomach. The type of this vomiting we meet with, according to M. Louyer-Villermay, in certain antipathies of the stomach, which occasion it to reject particular aliments, that the appetite of the patient may even crave. Another idiopathic nervous vomiting is that produced by viewing another in the same act. *Sea sickness* Dr B. is at a loss whether to consider as essential or symptomatic;—he cannot say whether the motions of the vessel by which it is produced act immediately upon the stomach, or affect it secondarily through the medium of the brain;—he is convinced, however, that it results from a morbid condition, either primary or sympathetic, of the nerves of the former organ. He conceives that inflammation of the stomach may be caused if the vomiting endure for any length of time; but he considers this as a very uncommon occurrence, sea sickness being very seldom attended by any serious consequences. Nervous vomiting resulting from any of the preceding causes offers no difficulty in its diagnosis; but in other cases, it may be confounded with vomiting from foulness of the stomach, from gastritis, from scirrhus of the stomach, &c. and this arises from the variations to which its symptoms are subject. This species of vomiting is, however, always more easy and is attended with less pain than that from any other cause. Immediately after it has ceased the patients feel themselves perfectly alert, and able to attend to their business, or they may even satisfy the calls of their appetite, which latter is seldom impaired. Dr B. considers as nervous vomiting, that which takes place in the first period of cholera morbus; that which is occasionally met with in hypochondriasis and hysteria; that of pregnant females; and that occasioned by a lesion of those organs with which the stomach readily sympathizes, as the intestines, the brain, the kidneys, the uterus, &c. When the lesion by which the stomach is sympathetically affected is a phlegmasia, Dr B. thinks with M. Gaultier de Claubry, that the vomiting soon changes its nervous for an inflammatory character.

Malacia. Pica. The first is a gastric neurosis which frequently accompanies gastralgia, but which may also exist alone, that is, without pain of the stomach or other nervous phenomena. The other species of perverted appetite is observed, principally, in deli-

cate children, in girls affected with chlorosis, and in pregnant women. In the first instance it is an idiopathic disease; the author thinks that this is also the case in the second; but in the third, it is evidently symptomatic. As individuals labouring under malacia swallow, often, great quantities of unwholesome and disgusting substances without inconvenience, Dr B. concludes, therefore, that their digestion is unimpaired, and consequently, that there cannot exist an inflammation of the stomach. In the case, also, of pica, the deprivation of appetite is unattended with bad effects, and disappears of itself, after the removal of the cause by which it has been produced, which is, also, he urges, a proof that it is unconnected with inflammation; for, he observes, genuine chronic gastritis does not disappear so easily, but frequently occasions the patient's death, a consequence never resulting from the neurosis under consideration.

Anorexia. Although the appetite is often diminished, and frequently perverted, irregular or augmented, yet it is a singular circumstance, and one of great importance in the diagnosis of the neuroses of the stomach, that we meet with few cases of gastralgia, hypochondriasis, nervous vomiting, &c. in which the desire for aliments is altogether destroyed, so long, at least, as these neuroses are uncomplicated with some other lesion. Hence idiopathic anorexia is of very rare occurrence.

Bulimia. Excepting when it results from a malconformation of the stomach, the opening of the biliary ducts into its cavity, the absence of the gall bladder, or any similar cause, bulimia is a pure gastric neurosis, and may exist either by itself, or as a symptom of gastralgia.

"After having taught," says our author, "that an excessive appetite produces an inflammation of the gastro-intestinal mucous membrane, and that the nervous erethism, in which this appetite consists, is nothing else than the incipient stage of a gastro-enteritis, the physiological physicians nevertheless cause the persons whom they believe to be menaced, or already affected with this inflammation, to refrain from food, we had almost said allow them to die of hunger; not perceiving that their practice is here in direct contradiction to their theory: for in supposing that an excessive desire to eat will necessarily produce a gastro-enteritis, of which the erethism, upon which this desire depends, is the commencement, the first indication to fulfil, in order to prevent its occurrence, or to arrest its progress, would be, of course, to appease the inordinate appetite by a sufficient quantity of food; it being a general rule in medicine to commence the treatment of a disease by removing the cause by which it has been produced, or by which it may be kept up. In the case before us, however, the physicians alluded to pursue a directly opposite course: instead of removing the cause in order to destroy its effects, they on the contrary continue it!"

This severity in regard to diet, according to Dr B. is only proper in cases of decided inflammation of the mucous coat of the digestive organs, and then, he remarks, we have little occasion to prescribe it, as the patients have seldom or ever any desire for food. So long as they retain their appetites, it is nearly certain that their disease is merely nervous and not an inflammation; and the best means to prevent the development of the latter, is to allow them to eat with moderation. He cautions us, however, against supposing that it is always by inducing inflammation of the mucous coat of the stomach that an inordinate appetite produces injurious effects:—in general, it is only by its exasperating more and more the nervous sensibility of the digestive organ that it becomes injurious, and so little does this exasperation partake of inflammation, that the gastralgia resulting from it may endure for years, disappear, and again return, at a period more or less distant, without being attended by phlogosis of the stomach.

If the inordinate appetite which so commonly characterises the gastric neuroses were really an indication of the existence of a chronic gastro-enteritis, our author conceives that every individual who experiences a great desire for food,—convalescents, for example, in whom the appetite is, ordinarily, insatiable,—must be considered as affected, or at least strongly menaced with this inflammation: again, if the first or slightest degree of inflammation of the stomach augment the appetite, it ought to increase as the inflammation becomes more intense; but it is a well known fact that a disgust for food is the most constant symptom of a phlegmasia, or of the slightest irritation of the mucous membrane of the digestive organs, and the appetite does not return until the disease has disappeared.

"To be consistent, it is hence necessary to admit that the erethism which occasions the canine appetite is not of the same nature with the phlegmasia or irritation which produces anorexia. In fact it is impossible to believe that these two opposite symptoms can arise simply from an inflammation, more or less developed, of the mucous coat of the digestive organs. They may, however, exist in gastralgia; for those diseases which consist in an exaltation, or aberration of the nervous sensibility give rise to many anomalies, and to phenomena the most dissimilar."

Dr B. does not, however, assert that the nervous erethism which produces a devouring appetite, never gives rise to a gastro-enteritis; on the contrary, he is persuaded that it occasions it frequently, particularly when other circumstances, at the same time, concur to produce such inflammation. But he maintains, as he asserts from

experience, that the erethism in question, like the gastralgia in general, of which it is only a variety, is more generally simple, and without any admixture of inflammation.

Our author here concludes his list of the individual gastro-intestinal neuroses, and after indulging in a long philippic against the doctrines of the physiological school, in the course of which we were sorry to remark one or two misrepresentations, he adds,

"If, contrary to every species of reason, it is absolutely insisted that the name chronic gastro-enteritis be given to the gastro-enteralgia, the adjective *nervous* ought, at least, to be added, to distinguish them from genuine gastro-intestinal inflammations. But especially, should the indiscriminate employment of antiphlogistics be abstained from in their treatment; these remedies are adapted only to the first period of certain cases, and even then, to prevent any injurious effects resulting from their employment, they are to be used with much greater moderation than in inflammation. The prognosis must, also, be modified; they are not to be pronounced, in general, fatal; such an opinion, however true in the case of an actual chronic inflammation of the digestive organs, is entirely false when applied to the neuroses of these parts. I do not say that these latter affections are exempt from all danger; by the obstacles they present to digestion, and consequently to nutrition, they may occasion a loss of strength, emaciation, languor, marasmus, &c. they may, also, become dangerous in giving rise to a gastro-enteritis, to disorganization of tissue, or to melæna or hæmatemesis. But such results are rare; they occur only under the following circumstances. 1. In individuals predisposed to them. 2. When these neuroses are very violent, recur frequently, or become constitutional. 3. When they are mistaken, and improperly treated, either by stimulants, which are capable of inflaming the stomach and intestines, or by antiphlogistics, the abuse of which frequently causes the death of the patient by a total destruction of the functions of the digestive organs."

We had intended to have tested, by the diagnosis of our author, the phenomena of those diseases, which he has referred to the class of gastric neuroses, as they are presented to us in practice and in the descriptions of standard authors; but having already occupied more space than was strictly our share, we must leave this task to our readers, fully persuaded that, when they have performed it, they will find themselves under the necessity, from the author's own showing, of removing the majority of these diseases out of the class of simple neuroses. We are not to be understood, as now offering any opinion as to their real nature; we must say, however, that the author has failed in establishing, to our satisfaction, their purely nervous character: did we even profess *ultra Broussaism*, we see nothing in the work before us that should shake our faith.

Although somewhat out of place, we must be permitted to notice here Dr B.'s *speculations* as to the cause of *ramollissement*, or softening of the stomach. He thinks that this morbid change is, at

least very frequently, to be ascribed to the abuse of diluent and mucilaginous drinks, by the physiological school, in the treatment of diseases of that organ. "It is certain," he remarks, "that a portion of mucous membrane placed in water soon becomes softened and decomposed." He admits, however, that there is some little difference between a portion of dead membrane and a living stomach: but, he asks, may not a period arrive when the vital properties of its tissues will be forced to give way to the continued imbibition of diluents? In support of this opinion he calls in the aid of the two physico-vital actions (*exosmosis* and *endosmosis*) described by Dr DUTROCHET*; in virtue of which it is supposed that the fluids contained in hollow organs, as well as those which surround them, penetrate and traverse their parietes.

"Now," says our author, "this passage of fluids through organic parietes, which may be compared to an infiltration, cannot be effected without causing those parietes to undergo a species of ramollissement, which by frequent repetition may amount, finally, to a true disorganization."

We proceed next to the treatment of the gastro-enteralgia. This consists principally in a proper regimen, and the administration of "the *fixed tonics*, devoid of every stimulant principle." If there be circumstances which render the latter injurious during the first period of these affections, they are almost always necessary in the second,—other remedies are, for the most part, only palliatives; it is, we are assured, a tonic treatment, alone, seconded by a proper regimen, that will produce a radical cure of the gastric neuroses. When, however, the case is obscure, or its symptoms equivocal, so that it is difficult to determine whether it be a chronic gastro-enteritis or an enteralgia, nature is to be interrogated by the cautious administration of antiphlogistic remedies; if the symptoms be ameliorated by them, it is a proof, according to our author, that the case is one of inflammation, and *vice versa*. It is possible, however, that this proof may not be decisive, for antiphlogistics will calm, likewise, a violent nervous erethism, but then an inflammation of the stomach is threatened, and the remedies adapted to such inflammation are indicated. If, in the same manner, we commence the treatment under an impression that the case is one of atonic gastralgia, and the disease is aggravated, the remedies are to be immediately laid aside for others of a more appropriate nature.

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* See a review of Dr DUTROCHET'S Recherches in our last number, p. 92.

In regard to diet; patients affected with gastralgia may indulge their appetites without danger; they are not to eat less than what they were ordinarily accustomed to, excepting when they reject their food by vomiting, which rarely happens. Those who have no appetite should confine themselves to a small quantity of aliment, unless it be immediately rejected. When the desire to eat is considerable, as in bulimia, they are to be cautious not to satisfy it to its full extent. They should be regulated by their ordinary habits, and not take more nourishment than they did when in health, whatever may be the voracity of their appetite. In all cases, they are to take their food at regular periods; but not exceeding three meals per diem. They should, particularly, avoid satisfying any desire to eat which may be experienced during the night, as such desires are, most commonly, unnatural. With respect to the kind of food proper in gastralgia, all such articles as are too debilitating or too cooling, as well as those of too exciting a nature, are to be avoided. Thus on the one hand, every species of aliment, whether vegetable or animal, in which an aqueous, fatty, mucilaginous or acid matter predominates, as well as all those which contain too much of a stimulant principle, are to be proscribed. In the gastro-enteralgia from erethism, food of the mildest nature is alone proper, such as chicken or beef broths thickened with oaten bread, crust of bread, Brussels biscuit, rice, or Indian meal; weak soups thickened with the same, with the addition of sugar and the yolk of an egg; the white meats boiled or roasted; fresh eggs; light fish; those fruits and legumina which abound in sugar and fecula, or at least, which contain but little water or mucilage, and for drinks during meals, water merely reddened with very old Bourdeaux or Burgundy wine; when the wine disagrees with the patient, sugared water is the most proper drink. The aliments proper for a case of atonic gastro-enteralgia, must be of a somewhat more strengthening character; the principal of these are animal jellies, osmazome, mutton or beef roasted, *légumes au jus*, wine and water, &c.

Of the remedial treatment. There are three cases in which the abstraction of blood, either general or local, will be useful or even necessary. 1. When the pain is excessive, and capable by its violence of producing gastritis. 2. When the patient is plethoric. 3. When the gastralgia has been preceded by the suppression of an habitual hemorrhage. In all others, even the moderate use of blood letting is useless, and its immoderate use always produces bad effects.

Mucilaginous drinks, particularly gum water, are invariably injurious. In certain doubtful cases they may be even regarded as a test to distinguish gastralgia from gastritis :—when under their use the disease increases, we may conclude that it is nervous, or at least that a morbid state of the nerves is chiefly concerned in its production.

When the neurosis is indolent or attended with little pain, medicines are not always required,—it is sufficient for its cure, that its cause be removed, and the patient placed under a proper regimen and moral treatment.

The best medicinal drink in gastralgia from erethism, whether painful or not, is veal or, still better, chicken water. In many cases, but particularly when there is no thirst, Dr B. directs a jelly made from the flesh of a chicken, in the quantity of a coffee spoonful at a time,—milk and water is also proper when the milk does not offend the stomach. The only vegetable ptisans at all admissible are a decoction of barley or oats, of chiendent, liquorice, rice or bread,—the two first may, however, be injurious, because of too cooling a nature. With these, certain external means are useful; as tepid bathing, or cataplasms of rice or crumb of bread, or fomentations over the stomach. The foregoing treatment is not to be too long continued, for fear of an atony of the nerves being produced. The moment it ceases to be of advantage, it should be associated with tonics; such as chicken or veal broth, with the addition of orange leaves, hops, dandelion, wild succory, germander, &c. &c., or a decoction of barley and cinchona, or of rice with the addition of a small quantity of catechu; or as a more simple prescription, we may direct the cassia lignea, the lichen islandicus, or acorns, roasted, ground, and made into a kind of coffee; Dr B. has known dyspepsias cured by the use alone of the latter. The tepid are to be laid aside for cool baths; and tonic substances, as oak bark, are to be added to the cataplasms or fomentations.

The extreme sensibility of the stomach being calmed, if medicines appear to be yet necessary, the tonics are to be continued by themselves. The best are, wild succory, rhubarb, wild valerian, gentian, carduus benedictus, colomba root, absinthium, lesser centuary, cinchona, ox-gall, the preparations of iron, Seltzer water, ice or iced water, cold or medicated baths, and to the epigastrium, a plaster of theriaca and assafœtida.

In the treatment of atonic gastro-enteralgia, we are to commence

with the mildest tonics, and gradually pass to those of more power. When there is much increase of sensibility they are to be employed in conjunction with remedies of a soothing nature.

In all cases of gastro-enteralgia accompanied with augmentation of sensibility, or with pain, sedatives become necessary. Under this head the author includes ice or iced water, taken internally,—this is improper, however, in cases of decided erethism, or when a gastritis is to be feared. Among the anodynes, opium is the one the best adapted to cases of gastralgia :—for the rules to be observed in its use, and the comparative value of the other narcotics, we must refer to the work itself.

Constipation, when long continued, is to be overcome, if possible, by a proper diet; or, this not succeeding, by injections; but they must not be often repeated, nor must those in common use be employed. Water, rendered somewhat tonic by the addition of *cassonade* or sugar, injected of a tepid warmth in cases of atony, and cold in cases of erethism, is recommended by the author. Muriate of soda he forbids ever to be added to the injections.

When a vomiting accompanies the gastralgia, *l'Eau de Vichy*, magnesia, bicarbonate of soda, &c. may be of advantage. If there be a foulness of the stomach, neither tartar emetic, nor purgatives, will be proper; a reduction of the diet, or even entire abstinence, will generally be sufficient to remove such symptom, if not, a cautious administration of ipecacuanha and the mild laxatives, with the exception of honey, is to be resorted to. The *poudre aérophore* of Schmidtmann will also be of advantage. The composition of this powder is as follows: Carb. sodæ, one part, tartaric acid, one part and a half, white sugar, one part. To be dried by a moderate heat, well mixed, and kept in a closed vessel.

The *nux vomica* is recommended by Schmidtmann in cases of neuralgia of the stomach. Dr B. restricts its employment to extreme cases, when other remedies fail. The *oxyde of bismuth*, Dr B. conceives to be a doubtful remedy. *The essential oils, antispasmodics, &c.* are always of equivocal utility, and liable to produce dangerous effects. Revulsives, externally applied, exasperate rather than remove the pain of the gastro-enteralgia. Blisters, particularly, are proper only when the attack has been preceded by the repercussion of some cutaneous disease, or of gout, or rheumatism.

The author's chapter upon *moral treatment, occupation, exercise, country air, &c.* contains, undoubtedly, many very judicious pre-

cepts; they are such, however, as will occur, at once, to the mind of every well instructed practitioner; we pass them by, therefore, without attempting an analysis.

The treatment of the gastro-enteralgia complicated with chronic gastro-enteritis, is to be effected by a proper *combination* of antiphlogistic and tonic remedies. But *combined* in such a manner that the first shall form the basis of the treatment, if the case commenced as a gastritis: "while we insist more upon the gradual and cautious employment of the second, when the neurosis has been the primary affection."

We cannot conclude our review of the work of Dr Barras without a remark or two upon the spirit in which the greater part of it has been composed, and which, we regret to say, is in the highest degree reprehensible. Notwithstanding the author in his initial paragraph admits that "the physiological doctrine has rendered important services to our science," and notwithstanding the praise he has bestowed upon the *Histoire des Phlegmasies Chroniques* of its founder, we find him, subsequently, representing the doctrine *en masse* as having retarded the progress of medicine, as destroying the sick, raising up imaginary evils to afflict those in health, as sinning against physiology, in short, as a doctrine not worth the trouble of studying. But this inconsistency is not the most serious part of the charge we bring against Dr B.—"*est huic diversum vitio vitium prope majus*"—we allude more particularly to his frequently representing the physiological physicians, not as men honestly mistaken in their medical opinions, but as entirely devoid of moral principle, as misrepresenting or concealing facts, manufacturing morbid appearances in support of their theory, and as willing to destroy their patients rather than acknowledge themselves to be mistaken in their diagnosis. These features would have deterred us from any notice of the work; but, it being one frequently referred to, by its title at least, as a complete refutation of many of the doctrines of the new school, we believed it our duty to extend a knowledge of its contents as far as the circulation of these pages will permit.

Φ. Κ.

ARTICLE XI.—*Medico-Chirurgical Transactions, published by the Medico-Chirurgical Society, London. Part II. Vol. XIII. Longman, Rees & Co. London*, 1827.

The first part of this volume we had the satisfaction of analysing for the first volume of this Journal. The second part, the substance of which we now propose presenting to our readers, contains many valuable facts, but is certainly deficient in good essays and in original observations. Perhaps, we may be too exorbitant in our demands; but from the *London* Medico-Chirurgical Society the profession have certainly a right to look for large contributions, not only to the facts, but to the principles of medical science. The former have multiplied exceedingly, and form, it must be confessed, rudis indigestaque moles; it is quite time for the talents of our brethren, especially in the crowded metropolitan cities of Europe, to bring order out of confusion, and thus to elevate the profession in character and usefulness. With fewer opportunities for improvement, American physicians have achieved much, and are still actively and laboriously engaged in the prosecution of the science, as well as the practice, of medicine. Facts, however, cannot be too numerous, and we shall very summarily and with few comments present those which the present volume contains.

The first paper, from DAVID GALE ARNOT, Esq., records the case of a soldier, aged 44 years, who was wounded in 1812 by a musket shot in the left side. He suffered from occasional attacks of inflammation of the lungs from exposure to wet and cold, and eventually died, Feb. 23, 1826. On dissection, the left lung was found completely hepatized; some pus was observed in the bronchiæ and trachea; and, opposite to the third rib, a cyst was discovered, containing "a piece of iron-hoop about an inch in length and of the form of a crescent." The other lung was healthy excepting some slight pleuritic adhesions.

The case confirms what physicians and surgeons have been slow in believing, that the parenchymatous substance of even vital organs has comparatively little irritability; their inflammatory affections being often very destructive and lasting for a long time, in the present case for fourteen years, without exciting very severe sympathetic affections.

This remark is further illustrated by the ensuing paper from Dr Rogers detailing the history of a case of injury of the head, with remarks by Mr Tyrrell.—Mark George, æt. 19, was wounded in the forehead by the bursting of a gun on the 10th of July 1825. He was able to walk some hundred yards immediately after the accident, but at length, *overpowered* by the loss of blood, he fell and shortly after became convulsed, and was totally deprived of his senses. [Were the convulsions, &c. the effect of the loss of blood, or of effusion within the cranium ?] The external wound being enlarged, exposed an orifice about the size of a crown piece in the frontal bone, just above the centre of the left superciliary ridge, through which a considerable quantity of the substance of the brain was oozing, and as much as a table-spoonful adhered to the hair and surrounding integuments. No foreign substance or spicula of bone was discovered. The wound was lightly dressed, and the patient bled to ℥xvi. with advantage. Next morning, would answer questions. Took cathartics.

July 12. Symptoms favourable. Pulse, although not exceeding forty, was tense and full. V.S. ℥xvi. Cathartic.

July 13. Pulse quicker and softer. No fever. Seems quite rational. A poultice applied to the wound; a small pledget of cerate was placed over the orifice in the bone.

July 21. Little change in the symptoms since last date. Now some irritation and febrile heat. V.S. ℥xii. Cold ablutions with vinegar and water.

July 22. Improved. Wound dressed as before. Fœtor very great, and discharge copious. Surrounding parts have a healthy aspect.

July 27. Discharge continued very abundant and symptoms very favourable until this day: has now some pain in the head—greater reluctance to answer questions; stiffness of the lower jaw; bowels costive.

July 28. Bowels relieved by a purgative: is better, the stiffness of the jaw gone off.

July 29. Takes more nourishment; still some sense of choaking in the throat and difficulty in swallowing—a little inclination to drowsiness, but is perfectly sensible; pulse sixty, soft and regular. Discharge from the wound is profuse.

August 1. No difficulty of deglutition: took a draught of port-wine and water; broth, &c.

Aug. 2. Discharge profuse and strength lessened, but by improving the diet and giving cinchona, no other unpleasant symptom followed.

Aug. 4. A foreign body was discovered in the cranial opening, but so impacted as not to be removed by the forceps.

Aug. 5. By means of a trephine elevator and a forceps, the foreign body was removed, and proved to be the iron breach-pin of the gun, three inches in length, and exactly three ounces in weight, of an irregular shape, cylindrical at one extremity, the other pointed. It had thus remained under the cranium for twenty-six days.

Aug. 16. No bad symptoms supervened; some small portions of bone been discharged. Was this day removed to his own home, about a mile distant, without injury.

Aug. 25. Cavity filling up. Able to sit up.—This boy continued to improve without any unfavourable symptoms of mind or body. Small portions of the bone exfoliated occasionally as late as the 20th of November. On the 10th of December he is reported as in good health.—The sight of the left eye is totally destroyed, but that of the right remains perfect. The smell and hearing are both perfect, and his memory and other mental faculties have not been evidently impaired. There does not appear to be any difference between the two sides of the face and head in point of sensation or power of motion.

Mr Tyrrell's observations do not increase the value of the above communication. He considers as "an extraordinary feature in the case, the trifling effect occasioned by the removal of the breach-pin," he would "have expected considerable irritation and inflammation to have followed." Upon what principle, however, is not very evident; for surgeons have always acted on the principle of removing foreign substances with the view of diminishing irritation, whether such bodies have been resident in a part for a long or short period. Mr Tyrrell's fears are analogous to those of some late pathologists who are afraid of cathartics in gastro-enteritic irritations caused or kept up by acrid ingesta: but *causa sublata*, &c.

Dr Forbes presents us with a case of inflammation of the iliac and femoral veins in a consumptive patient, with an account of the post mortem appearances. It is not very satisfactory. The patient suffered much pain in his left lower extremity, which was tense, painful and pale as in phlegmasia dolens with an enlargement of the subcutaneous veins. On dissection the cellular tissue of the whole

limb was found much infiltrated with a limpid fluid, and the veins of the limb, including the external and common iliac, were greatly distended with blood. The common iliac seemed as large as the inferior vena cava; the distention of the vessel suddenly terminating where it is united with the right common iliac vein. The veins on the right side were nearly empty, and quite healthy in their appearance. The coagulated blood in the left iliac and femoral veins was of a much firmer consistence than usual, and of a more fibrous appearance. "On separating this coagulum, a thin but distinct membranous layer was seen adherent to the internal coat of the vein, and it required some force to separate them." The distention of the veins with blood and this thin membranous layer were therefore the only morbid appearances in these vessels. They are not sufficient, we think, to justify the inference, that inflammation of the veins was the *cause* of the previous pain, swelling and infiltration of the limb; for dissection revealed very slight evidences of inflammation in a complaint which had lasted for a considerable period (its actual duration is not given); and we have in the common varicose condition of the veins, and in many other cases of acute and chronic phlebitis, phlogosis of a much more severe character without the extraordinary infiltration of the cellular tissue here recorded. Moreover, what evidence does the distention of the vessel afford of inflammation? If we were left to make our own inferences from the facts of the case, we should certainly regard the distention as the cause of the inflammation, not its consequence, and should refer the painful and dropsical condition of the limb to this accumulation of blood rather than to the phlogosis of the lining membrane of the iliac veins. Phlebitis is generally characterized by a thickening and induration of the parietes of the vessel, and even where the inflammatory action is confined to the internal tunic, the quantity of blood is usually much diminished, not increased. Great attention is requisite to distinguish the causes from the consequences of disease when making inferences from post mortem appearances. A caution very requisite at the present period, when rather an undue importance is awarded to the labours of the morbid anatomist.

The next paper is entitled a "case of hydrophobia, and the appearance of the body on dissection fourteen hours after death, with some remarks on the nature and treatment of the disease. By Anthony Todd Thomson, M.D. F.L.S. &c."

The case is well detailed and very interesting. We can only mention that the subject was a boy, eight years and a half old, in whom the disease appeared forty days after the patient had been bitten on the hand by a rabid cat, and twenty days after the cicatrization of the wounds made for extirpating the injured parts. The excision was performed the day after the accident. The treatment adopted by Dr THOMSON mainly consisted in the exhibition of mercurial cathartics and of the prussic acid, assisted by opiate frictions. The prussic acid had an evident influence, temporary indeed, in moderating and even in occasionally arresting the convulsive movements of the unhappy patient. Sixteen drops of this medicine were given in the course of eighteen hours. The child died at 2 o'clock A.M. November the 10th, 1826, after an illness of three days.

"*Dissection of the Body.*—The body, externally, presented no unusual appearance. There was a greater rigidity of the extremities than usual, and the body exhaled a peculiar fœtor. The first object being to examine the spinal cord, an incision was made from the nape of the neck to the loins, and the vertebræ on each side of the spinous processes being sawn through, the cervical portion and part of the dorsal portion of the medulla spinalis were exposed. The whole of the cellular membrane between the theca vertebralis and the parietes of the canal was loaded with suffused [?] blood, which in several places lay in black coagula. Six inches of the cord, from the third cervical vertebra to the fifth dorsal, being removed and the dura mater slit up, the vessels of the spinal cord were seen turgid, particularly those which accompany the dorsal nerves.

"On opening the cranium, the meningeal vessels were more turgid with blood than usual, but certainly not so great as to indicate inflammation; nor were there any bloody points seen in the divided substance of the brain, such as present themselves in states of previous great excitement of that organ. The ventricles were empty; the choroid plexus rather more turgid and darker coloured than common; and, on the base of the brain, particularly in the pia mater covering the pons Varolii and the medulla oblongata, there were evident symptoms of inflammation having affected that membrane. The lungs were gorged with dark coloured blood, but in other respects healthy; as were also the heart and pericardium. The vessels accompanying the left phrenic nerve were turgid with blood; those of the right perfectly natural.

"The larynx displayed a slight blush, which deepened on the outer surface of the epiglottis, and still more so in the inner surface. The lining membrane of the trachea was also slightly redder than usual. The pharynx shewed evident traces of inflammation; but there was no unhealthy appearance in the œsophagus. There were several patches of ecchymosis in the stomach; the liver, spleen, pancreas, and intestinal canal were apparently in a healthy state."

In the learned *remarks* which Dr THOMSON has appended, he examines the two questions "whether any change of action, sufficient to generate a poison similar to that left in the wound, takes place in the vessels of the part?" and "whether the virus be absorbed into

the system immediately before the accession of the disease ?" In the absence of direct evidence in support of the affirmative of either of these questions, Dr T. inclines to the negative, as more consistent with the phenomena of the disease: and he would hazard the following conjecture, that the virus remains dormant in the part, until a certain state of habit renders the nerves in its vicinity susceptible of its influence; and this being communicated, a morbid action is begun in these nerves, and *extended to the respiratory nerves which induce the whole train of symptoms constituting the disease.* Hence, the sense of suffocation, as well as the difficulty of swallowing fluids, the gorged state of the lungs "which is a universal appearance noticed in every recorded dissection of individuals who have died of hydrophobia," the sobbings, the convulsions, &c.—may all be referred "to a morbid change in the pons Varolii, the medulla oblongata, and the medulla spinalis, communicated to those nerves which originate in these parts, and which have justly been denominated, from their functions, the respiratory nerves." In confirmation of these views, he appeals to the morbid appearances observed post mortem, by himself and by various other anatomists, some of whom he quotes, showing an inflamed or at least injected condition of the membranes or fibres of the pons Varolii and its prolongations, and of the nerves proceeding from these bodies.

The *treatment* resulting from this pathology would consist, first in diminishing excitement in the medulla oblongata and spinal cord by removing every source of irritation internal or external, and second, as soon as an intermission is effected, in promoting the tone of the system as rapidly as it can be accomplished.

"With these objects in view, I would propose to cup the patient repeatedly along the whole course of the spine, and to follow this with blisters on the same part; to exhibit large doses, say twelve or fifteen grains, of calomel, combined with tartar emetic, whilst the strength remains unimpaired; both with the view of clearing the primæ viæ, and of obtaining the sedative effect which calomel in such doses produces. I would then recommend the administration of the prussic acid, in full doses; for independent of the benefit, at least to the comfort of the patient, which I have witnessed from the use of this powerful sedative, I am induced to look for more permanent advantage from it, by knowing the success which attended its employment by professor BRERA, in rachialgitis. With respect to the tonics to be administered for answering the last indication proposed, the sulphate of quinine, from its power and the small compass in which it can be given, is likely to answer better than any of the bitters or the cinchona in substance. It might be alternated with the nitrate of silver, which experience has proved to be a most powerful anti-spasmodic, as well as tonic, in chorea."

A case of extraordinary enlargement from an ovarian dropsy is next presented by Dr Charles Thomas. The patient Ann Payne was 52 years of age. She first observed a swelling in the left iliac region about twelve years ago, which continued to increase, so that on the 5th of July 1825 she measured, round the most prominent part, fifty-four inches, and from the extremity of the ensiform cartilage to the pubis, twenty-six inches. She was frequently tapped after the above date to the 31st of March 1826. The operation performed on that day was followed by some pain and fever. She became prostrated, and died, not from inflammation, Dr T. believes, but from the distention being too great.

Post mortem, the abdomen was found occupied with an immense tumour excepting a small space on the upper and lateral portions. There was some effusion into the peritoneal cavity; the peritoneum was about a quarter of an inch in thickness and of a cartilaginous character. The parietes of the ovarian sac varied from half an inch to an inch in thickness and contained about twenty-two quarts of coffee coloured fluid. A large mass of hydatids occupied the lower part of its cavity.

"It was the left ovary which had acquired this enormous bulk. It was fixed to the lateral ligaments of the uterus by a pedicle of peculiar firmness, and about an inch in diameter. *It had no other attachment whatever.* The uterus and right ovary were in no respect different from their usual state in healthy women of her age."

The substance of a communication from the experienced Merriman, respecting the duration of pregnancy in the human female, will be found in the Quarterly Summary of the last Number of this Journal, p. 169. It is presented in a tabular form.

Mr Norman records the case of an extra-uterine pregnancy, in which the fœtus was delivered through an incision in the posterior part of the vagina, where the head was distinctly felt. Before delivery could be effected, the cranium had to be perforated. The umbilical cord having unfortunately ruptured during the extraction of the child, no guide existed to the seat of placenta. This body was therefore retained. The patient's strength soon yielded, and on the day after delivery she died.

Dissection confirmed the account above given. The whole of the peritoneum bore marks of inflammation, but there were no adhesions. The placenta was found attached to the right ligamentum

latum, from which it was easily separated. The os uteri was situated above the pubes, as in retroversio uteri, and the vagina was so compressed that the finger could not be passed so as to feel the os tincæ from the vagina. The uterus was studded with soft tubercles, its substance thickened but not scirrhous, and its cavity was lined with a membrana decidua.

The two succeeding papers record three cases of rupture of the uterus during labour; in one the patient survived nearly eight weeks, the other two patients recovered. The two first are recorded by William Birch, Esq.: the third by Dr Smith of Maidstone.

Case 1. A female, aged 25, had two children previously; the first was delivered with the forceps, the last with the crotchet. On February the 21st 1827, she was again in labour; at 9 o'clock, the next morning, the uterus ruptured, and the patient was soon after delivered with the perforator and craniotomy forceps. Inflammation of the abdomen caused her death on the 7th of April.

Dissection exhibited adhesions of the intestines.

"The peritoneum lining the cavity of the pelvis, and the lower part of the cavity of the abdomen, was of a dark colour, exactly resembling the pigmentum nigrum, but it had lost none of its polish, nor was it in the least more tender than natural. This appearance I had never before seen, and it gave me the notion of a stain from the long contact of blood, or of some remaining particles of blood, which the absorbents had not yet had time to remove. However, I am told by Mr Stanley, such an appearance of the peritoneum is every now and then met with as an effect of inflammation."

There was a contraction of the antero-posterior diameter of the pelvis; the uterus had regained its ordinary unimpregnated size; there was a longitudinal laceration, implicating the posterior part of the cervix uteri and the posterior parietes of the vagina, about one inch and a half long. The attempts at reparation were very trifling.

Case 2. Mary Wren, aged 29 years, the mother of three children; two were born alive, and one dead after a tedious labour. Labour again commenced on Sunday evening, April the 15th. The funis and a foot presented, and the head and a hand could be felt on Monday at the upper aperture of the pelvis. During a severe pain between two and three o'clock A.M. symptoms of ruptured uterus occurred, and the labour pains gradually diminished, so that soon after three o'clock they entirely ceased. The cranium was perforated with some difficulty as the head was moveable, but on attempting extraction, the

head receded over the pubis, and turning the child became necessary. Owing to a contraction of the pelvis, the head could not be extracted until it was again perforated behind the ear. The placenta, being in the vagina, was immediately removed. There was no hemorrhage. Mr Birch found a rupture on the anterior part of the uterus, through which his hand passed into the abdomen and came into contact with several coils of intestine. The contracted uterus was felt, encompassed with intestines and lying at the back of the abdomen.

Some abdominal inflammation supervened, and was treated with leeches, fomentations, poultices, purgatives, and effervescing draughts, and afterwards with calomel and opium, so as to affect the gums. She had a relapse at the end of three weeks from which she completely recovered, and at the end of seven weeks occupied herself in domestic affairs. Her menses returned, and on the 6th of September she was perfectly well.

From the history of cases of ruptured uterus, Mr Birch infers, that a fatal event is more common, when the rent is on the posterior portion of the uterus; and also, that when the child cannot be immediately delivered per vias naturales, gastrotomy is much preferable to the practice, recommended by high authority, of leaving the unfortunate female to all the dangerous as well as distressing consequences inevitably resulting from the residence of the dead fœtus in the peritoneal cavity. This opinion we think defensible on every ground, especially on that of experience; for so many cases have been recorded, especially of late years, of individuals surviving the Cæsarian section, as well as gastrotomy executed not only for the removal of the fœtus, but of ovarian tumours, &c. that the natural dread of such operations has been greatly diminished, and surgeons have become, scientifically and practically, better prepared for its successful execution*.

Case 3, of ruptured uterus, successfully treated by Dr Smith, presents some other points of practical importance. Mrs H. a healthy female about 40 years of age, the mother of nine children, suffered much during her last labour, two years since, but was eventually delivered without instrumental assistance. Considerable constitutional irritation followed, with some sloughing of the vagina

* Vide North American Med. & Surg. Journ. Vol. I. p. 30, 235, 236, 459. Vol. II. 205, 206, 439. Vol. V. 235, 236. Vol. VI. 197.

and bladder, and she has since been unable to retain her urine, which passes per vaginam. On the 16th of July 1825, labour again commenced during the seventh month of gestation. Pains were very severe and frequent; the os uteri dilated to the size of a crown piece but rigid, and the head presenting. Symptoms of rupture occurred about the middle of the day, and the pains of labour ceased. No part of the fœtus could now be recognized, and a rent could be distinctly felt, about two inches from the os uteri across the posterior part of the uterus, nearly three inches in length, through which two fingers readily passed. The edges of the rupture were extremely thin.

Turning was resolved on by the attending surgeons, Mr WHATMAN, Dr SMITH and Mr CHARLES, but its execution was impracticable at the time, owing to an adhesion of part of the os uteri to the vagina under the pubis which firmly resisted the passage of the hand, —the effect probably of the inflammation and sloughing resulting from the former labour. The symptoms became more urgent, and immediate delivery was essential for the safety of the mother. It was determined therefore to make a slight incision through the indurated part of the os uteri, so as to facilitate the passage of the hand. This was effected and the child delivered by the feet, after the head was perforated at the lambdoidal suture so as to allow it to pass. The placenta soon followed. There were slight uterine pains during delivery and very little hemorrhage.

The patient had well nigh perished during the delivery, and "continued in the most alarming state for almost two hours, great nausea, unconquerable restlessness, pulse running, and at times not to be felt, quick breathing, sighing, cold extremities, and Hippocratic countenance." Brandy and other stimuli were administered, and some reaction ensued. Symptoms of abdominal inflammation supervened, and were eventually overcome by antiphlogistics, a *cold* lotion (of what is not mentioned) being employed and proving very grateful and useful to the patient. By the 22d of July, a more nourishing diet was allowed and the cinchona administered; under which treatment convalescence was confirmed, and by the 19th of August the patient was able to ride out. A report, dated 23 March 1826, says that Mrs H. continues as well as before her last confinement.

"On dislocations of the vertebræ, by W. LAWRENCE, F.R.S. Surgeon to St Bartholomew's Hospital, &c."—This is a very interesting

and learned paper, embracing a review not only of cases and opinions of this comparatively rare accident, but of what has been recorded respecting inflammation, caries and anchylosis of the superior cervical vertebræ. Our analysis will be confined chiefly to the latter, after giving a summary view of the author's reading and observation on dislocations of the vertebræ. Baron BOYER doubts whether a luxation of the bodies of the vertebræ can occur without a fracture: DELPECH maintains that they are never luxated: and Sir A. COOPER observes, that "if luxation of the spine ever occurs, it is an injury which is extremely rare." On the contrary, RUST refers to cases in which even the lumbar and dorsal vertebræ have been dislocated: and Mr BELL records a case of complete dislocation between the last dorsal and first lumbar vertebræ, with entire division of the spinal cord.

From their anatomical arrangement, the cervical vertebræ are most liable to luxation, especially of their articular processes. DESSAULT and BOYER mention cases: and in the museum of St Bartholomew's hospital, are preserved several specimens of vertebræ more or less displaced. Mr LAWRENCE details a case which came under his notice, showing "that a complete dislocation, both of the articular processes and body, without fracture, may occur in the cervical region of the spine." The accident occurred in a young healthy man, who, while carrying a heavy barrel on the back of his head and neck, slipped in descending some steps, and fell on the buttocks, the burthen resting on the head and upper part of the neck. The patient died early on the fourth day. On *examination* no displacement or irregularity could be discovered until the soft parts were removed; when the body of the fourth cervical vertebra was found completely detached from that of the fifth vertebra; the inferior articular processes of the former were completely dislocated forwards; the yellow ligaments, and the fibro-cartilage between the bodies of the bones, were torn through; and the antero-posterior diameter of the vertebral canal was lessened about one third;—a state of parts readily explaining the occurrence of paralysis and the consequent death of the patient from imperfect respiration.

Anchylosis of the vertebræ, the consequence of previous inflammation of the cartilages and bones, with more or less encroachment on the spinal canal, and sometimes even with dislocation, may occur—but in such cases, death is the most frequent termination. Little notice has been taken of this disease of the cervical vertebræ

by English or American authors. Mr LAWRENCE gives us the outlines of a case which occurred to Mr WIGAN with a minute account of the appearances after death from a subsequent disease of the lumbar vertebræ.

At the age of five or seven, a child became the subject of an illness supposed to be hydrocephalus. After some time a swelling, obviously containing a fluid, appeared on the side of the neck and slowly increased in size. Pressure on this swelling affected the brain, producing a state of coma. The child used to move its head cautiously and slowly, supporting it with the hands at the sides. After a long continuance, the swelling and all the symptoms entirely disappeared. At no period, had there been any interruption or diminution of sensation or voluntary motion; and the recovery of health and activity was complete, the child being able to engage in the active sports suited to its age. There was nothing peculiar in the position of its head. After some time, disease of the lumbar vertebræ, with bending of the spine and abscess, came on, and the child died at the age of twelve.

Dissection exhibited the dentiform process of the second vertebra covered by the dura mater and projecting into the cranium on the right side and in front of the foramen magnum. After maceration there was exhibited an extensive displacement of the occiput, atlas and axis, and a firm consolidation of these bones in their new relative positions by the complete bony anchylosis of several articulations. The atlas is partially dislocated towards the left and thrown a little forwards and upwards; hence the right and posterior part of its bony ring diminishes the spinal canal. A considerable part of the right side of this bone has been destroyed by absorption. The axis is completely dislocated from the atlas and occiput to the right, so that its left portion intercepts about one third of the spinal canal, and the dentiform process projects by its whole length, between five and six eighths of an inch, into the cavity of the skull. The lateral displacement of this process is also great; the distance from the left anterior condyloid foramen to the middle of the basis of the dentiform process being seven-eighths of an inch, while on the right side, the distance from the right foramen to the same point is only two-eighths. No injury appears to have been sustained by the medulla oblongata passing over this projection. The bodies of the second and third vertebræ were displaced to the right. The right occipital condyle, the remains of the right transverse process of the

atlas, the inferior articular plane of the axis, and the right side of the basis of the dentiform process were anchylosed so as to form one solid bony mass. The left occipital condyle is partially anchylosed with the atlas; the left articular plane of the axis, its transverse process, and that of the third vertebra were all consolidated with the partial anchylosis just mentioned. The articular processes of the axis and the third vertebra are anchylosed, the union extending on the left side as far as the basis of the spinous processes. There is no appearance of caries. The dimensions of the vertebral canal were greatly diminished. The antero-posterior diameter of the foramen magnum was in this case one inch and a half, and the greatest measurement from side to side is one inch and a quarter. The lateral diameter of the ring of the third vertebra is seven-eighths of an inch, from front to back six-eighths. The measurement of the canal between the displaced portions of the atlas and axis is half an inch from before backwards and five-eighths of an inch from side to side.

This disease of the cervical vertebræ, so rarely noticed by the English authorities, has been fully described by professor Rust, of Vienna, in the Salzburgh Medico-Chirurg. Journal, 1813, Vol. XIII. and afterwards in his Arthrokakologie, or Diseases of the Joints, published in 1817. He thus describes the symptoms—

"Pain in the neck, becoming more severe at night or in swallowing a large mouthful or drawing a deep breath, is the first symptom. This pain affects one side of the neck, especially when the head is moved towards the shoulder; it extends from the larynx towards the nape, and often to the scapula of the pained side. No external alteration is perceptible; but firm pressure on the region of the first and second vertebræ produces considerable pain, and thus points out the seat of the disease. The difficulty of swallowing and breathing, and the hoarseness increase, alternating with pain in the neck, which seems to fix about the back of the head, and becomes intolerable on moving that part. The head sinks towards one shoulder, the face being turned a little down; for in general the articulations are affected on one side only, and that was the left in seven out of nine examinations after death. If both sides are affected, the head will incline directly forwards. In this state, things continue for several weeks or months; and before worse symptoms come on, there is often apparent improvement, freer motion, and more natural situation of the head. But, the uneasiness in speaking and swallowing returns, the pain becomes more severe and extensive, the head falls a little backwards and sinks towards the opposite side. The patient feels as if the head were too heavy, and he carefully supports it with his hands, when he moves from the sitting to the lying position, or vice versâ. This may be considered as a pathognomonic symptom of the affection. Another symptom, which, at this period, shows the true nature of the disease, is a peculiar expression of pain in the countenance, which, combined with the position and stiffness of the head, constitutes so characteristic an assemblage of appearances, that it is enough

to have seen it once, in order to recognize it immediately. This look of the patient, which RUST has endeavoured to represent in an engraving, consists especially in a general alteration of the features, with a heavy motion of the eyes, and a dull melancholy expression of internal painful sensations. More active indications of severe suffering are observed whenever the head is moved.

"In the further progress of the case, noise is heard in the head; deafness, giddiness, cramps, and convulsions, partial paralysis, particularly of the upper limbs, loss of voice, purulent expectoration and hectic symptoms supervene. Generally no external change is observable, either in the neck or in the nape; and RUST observed, in one case only, swelling of the affected side, which broke and left fistulous ulcers. But the slightest pressure in the region of the three upper vertebræ is acutely painful, and sometimes, in the advanced period of the disease, a grating of rough surfaces is distinctly perceptible when the head is turned. The patient may continue for months in this helpless and painful state, and then dies, either from exhaustion and debility, or, which is more frequent, suddenly and unexpectedly."

This description of the disease by RUST, Mr LAWRENCE considers as indicating "originally ulceration of the cartilages proceeding to the destruction of the ligaments and to caries of the bones, and extending in various shapes and degrees to the neighbouring important parts." The state of the parts, examined post mortem, of course, varies greatly. REILL mentions that in one example "the occipital condyles, the bodies of all the cervical and of the four upper dorsal vertebræ were found in a softened state, while the articular surfaces of the condyles and the two first vertebræ were partly destroyed." The appearances of the diseased parts in a more advanced state are described by RUST.

"I have always found, in a larger or smaller quantity, matter between the pharynx and vertebral column; it has sometimes been of a good kind, sometimes sanious. The pharynx is more or less ulcerated, and the periosteum and ligaments of the first vertebræ are destroyed. I have always found the original seat of disease in the articulations of the occiput and atlas, and of the atlas and axis; caries has attacked the occipital condyles, more frequently the left than the right, the atlas and the dentiform process. Where death has come on suddenly, the diseased dentiform process has been entirely broken off, or blood has been effused from ulceration of the vertebral artery, or pus has been found in the chest. The neighbouring important organs may be more or less seriously affected. Serous effusion is sometimes found between the dura mater and the arachnoidea, and in the ventricles. In the vicinity of the foramen magnum, where the space for lodging the spinal cord is sometimes much contracted, the dura mater is found livid, ulcerated and sloughy, and the neighbouring parts of the brain and medulla more or less altered in appearance. I have placed several preparations, illustrating this affection, in the pathological museum of the Vienna general hospital.

"In some cases," says Mr LAWRENCE, "the disease appears to be confined to the articular surfaces. After destroying the cartilages and ligaments, and producing more or less extensive displacement of the occiput and two first verte-

bræ, it stops, and the process of reparation commences, ending in bony anchylosis, by which the displaced parts are firmly fixed in their new position. Yet, in these more favourable instances, there is great suffering during the inflammatory stage; the extension of active disturbance to the dura mater, and the neighbouring contents of the cranium and vertebral canal, may cause dangerous and alarming symptoms, while chronic abscess may be formed externally as in the case which I have related."

In estimating the degree of pressure sustained by the spinal marrow, allowance, says Mr L., must be made for the fact that this medullary cylinder does not occupy the whole canal of the spine; hence dislocations, at least spontaneous dislocations, may occur, as in the case above alluded to, in which no pressure is experienced by the spinal marrow, and of course no paralysis results.

Patients occasionally recover from caries of the upper cervical vertebræ. Rust has seen one case of recovery, although he intimates that all the cases under his care terminated unfavourably. Mr Lawrence, however, has collected a number of facts from Reill, Wynpersse, Daubenton and others, and refers to several specimens in the museum of St Bartholomew's Hospital, showing that anchylosis of the superior cervical vertebræ has not unfrequently occurred; and that the patients have eventually perished from other diseases.

Mr Lawrence recommends no peculiar treatment for these inflammations. Rest, assisted by the common antiphlogistic remedies, should be steadily persevered in, as in similar affections of other portions of the spine.

We have been induced to give this extensive analysis of Mr L.'s paper, as sufficient opportunities are not afforded in this country for a proper investigation of this comparatively rare complaint, which there is reason to believe is occasionally observed.

Two papers are presented to us on the propriety of destroying nævi materni by strangulation; the first is from Mr Lawrence, the second from Mr Anthony White. To this last gentleman credit is given by Mr Lawrence for introducing this practice several years since; it was suggested also by Mr John Bell and by Mr Wardrop. The advantages to be derived from the ligature, single or double according to the size of the tumour, relate chiefly to the danger of hemorrhage, by which patients have been destroyed when the knife has been resorted to. See Mr Wardrop's case in the Medico-Chirurgical Transactions, Vol. IX. p. 203. The subsequent cicatrix is

said also to be smaller than when excision is practised, and although much pain and irritation are excited by tying the ligature so as to strangulate the tumour, yet no bad consequences have resulted, and these symptoms immediately disappear when the ligature is removed at the expiration of twenty-four or thirty-six hours. The ligature may safely be resorted to in cases where the tumour is so large and the vessels so dilated that the extirpation by the knife would be wholly inadmissible. This method has proved more successful, according to Mr LAWRENCE, than any of the measures formerly suggested; than the caustic, lunar or vegetable, as proposed by Mr WARDROP; than cold, astringents or pressure; and more effectual also than tying the supplying arteries or even taking up the main trunk of the supplying vessels, an operation which, although it has been occasionally successful, promises very little for the radical treatment of these "erectile tumours," so named by DUPUYTREN. Interesting cases are given by Mr WHITE and Mr LAWRENCE, which we need not detail, especially as we have already noticed this operation in the fourth volume of this Journal, p. 425.

On the use of sulphate of copper in chronic diarrhœa, by JOHN ELLIOTSON, M.D.—We have, in this communication, the history of several cases of chronic diarrhœa and dysentery, in which the sulphate of copper, in doses of half a grain to one grain and a half, usually combined with half a grain or a grain of opium, was very effectual in restoring patients to health where all the usual practice had failed and when no other apparent hope remained for the sufferer. The opium was given to prevent nausea and irritation from the cuprous salt; that it however was not the efficient agent was shown by comparative trials of the two articles, and especially by the fact that the opium alone, or in combination with astringents, had previously failed in the same cases in which the pills of opium and copper succeeded. Several of the cases were of long standing, with every indication of great derangement of the chylopoietic secretions; the stools being often very copious and numerous, like a mixture of lime and water, frothy and of a sour smell; even when of some consistence, still pale, and when even they become figured and dark, lighter than in health, and soon becoming frothy and white. In other cases, the discharges were bloody or mucous, attended with pain and scalding. The sulphate had the effect of altering the character of the secretions, so that they became more natural in consist-

ence and colour, often quite bilious, and at the same time, the appearance and strength of the patient rapidly improved, and perfect health was eventually obtained in almost every case.

Dr Elliotson has hardly alluded to the pathology of chronic diarrhœa and dysentery, or to the modus operandi of his proposed remedy. On the former point, we are much indebted to the French school of medicine, whose laborious investigations of late years have shed great light on these subjects and entirely overthrown all preconceived hypotheses on the subject of passive exhalation and chronic debility in these wasting and exhausting diseases. Ocular demonstration is daily afforded to those who will look, that inflammation of the mucous tissue of the small or large intestines, in its various stages from simple erythema to complete ulceration and gangrene, constitutes the disease under which the patient's strength is so gradually and so frequently exhausted. The idea of imparting strength to an individual thus circumstanced, merely by exhibiting tonics and astringents, and especially solid nutritious aliment, is as absurd in theory as it is injurious in practice. So long as the strength of the patient is adequate, the common antiphlogistic measures, leeches, cups, fomentations, blisters and other revulsives, assisted by cold mucilaginous enemata and a diet strictly farinaceous, will answer if steadily pursued. When however strength is rapidly failing and when structural derangement is occurring, a more nourishing diet is demanded, but this should always be of a pulpy or liquid consistence; solid food, rapidly driven through the irritated alimentary canal, operates invariably as a *mechanical irritant*, aggravating all the symptoms, and bringing on relapses even in patients quite convalescent. Without a liquid diet, no medicine can be of much avail. This practice may then be assisted by those alterative measures which have been found so useful in other chronic *mucous inflammations*, as of the eye, urethra, &c. Hence the advantage derived in chronic dysentery from Moseley's vitriolic solution (of alum and sulphate of zinc), from pills of acetate of lead as recommended by Drs Harlan and Coates, from the nitrate of silver as lately advised by Dr Johnson in morbid irritability of the stomach and bowels, and no doubt also from the sulphate of copper as now proposed on experiment by Dr Elliotson. Clinical experience demonstrates, that such stimuli, long used advantageously in external inflammations, do not aggravate the phlogosis of the internal mucous tissues

when judiciously exhibited*. To the same test, we may appeal for confirmation of the practice of exhibiting suitable cathartics in almost every stage of bowel complaints. The disciples of the Broussais school will exclaim, "What! exhibit irritants when the disease is irritation!—will you add fuel to the devouring flame?" Cathartics of every kind may be termed irritants—but most of them are irritants of that peculiar character, that in almost every instance secretion is excited, so that large quantities of fluid are poured out by the inflamed vessels, and thus their excitement is reduced precisely on the same principles that a hot poultice relieves a phlegmonous inflammation. Purgatives are useful in bowel complaints, not only by exciting capillary secretion, but especially by removing the acrid, solid and fluid contents of the bowels,—often the cause of their phlogosis and always aggravating it however excited. This is an indication almost wholly neglected by the Broussais school, who tremble at the idea of the irritation from an ounce of salts, and yet allow the undigested fermenting or putrefying food and the scalding secretions of diseased tissues to remain, for days or weeks, in contact with the irritated mucous membrane. The irritation of the muscular coat of the intestines, excited by a cathartic, may be injurious, but experience shows that the injury must be very trifling and can form no objection to its employment when the above mentioned indications are to be fulfilled, especially as the regular peristaltic motion thus excited is much less painful than the irregular spasmodic contractions from acrid ingesta, bile, mucus, &c.

We have been led to these observations by two considerations; First, That the establishment of correct principles in medicine, principles legitimately deduced from the laws of the economy and from the modus operandi of the causes and remedies of disease, is all important, and will often, as in the present case, lead to scientific and successful practice. A second motive for this digression is to protest against hasty assumptions and illegitimate conclusions which

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* Influenced by the reasoning in the text, the writer of this analysis has resorted to injections of a solution of sulphate of zinc (grs. x.—xx. to ℥viii. of water) in these chronic inflammations, after the violence of the disease has been subdued and the symptoms of general excitement disappeared. The practice has been very successful, and on comparative trials has appeared to him more useful than when the same article has been administered by the mouth, as no nausea is excited, and the application is made directly to the inflamed surface. At some future period, cases of its efficacy may be presented to the public.

are opposed to careful experience. As may be readily observed, the opinions of the Editors of this Journal are different on many points,—a reason why they were appointed; but they are all *eclectics*, and not devoted to any school,—they wish and labour to test every opinion by clinical observation, and dread, as much as any lover of empiricism, the terrible consequences of unphilosophical system-making. They nevertheless are advocates for principles in medicine, and believe that more good than harm has resulted from the efforts even of those great minds whose imaginations have occasionally led them astray from true philosophy. Many who are too indolent to collect and collate facts, rail at theory as the bane of medicine, forgetting that theory constitutes the identity of medical science, without which empiricism—ignorant empiricism would reign triumphant; from which may we and our patients be preserved by a kind Providence.

In a postscript to his paper, Dr Elliotson states summarily the result of his experience with *quinine* in intermittents, and the *subcarbonate of iron* in chorea. The advantages of the former he fully confirms, and adds that he has with complete success given the quinine in intermittents, when complicated "with so much inflammation in the abdomen, chest or head, as to require venesection, some with dropsy, some with a chronic disease of the lungs or liver; *but every one was cured.*" He has been convinced that the best practice is to give ten grains as soon as the paroxysm is over, which will often be sufficient for preventing a return, but it is sometimes necessary, in addition to these ten grains after the fit, to make the whole quantity in the twenty-four hours amount to a scruple, or half a drachm, by small doses also, every six or eight hours.

The subcarbonate of iron in large doses was recommended by Dr Elliotson in the former part of this volume of the Transactions as very successful in *chorea*, eight cases of which he detailed (Vide N. Am. Med. & Surg. Journ. Vol. I. p. 455). He has since attended nine cases of the disease, *every one* of which yielded to the remedy. The time usually required varied from six to ten weeks; one was cured in four, and one in not fewer than twelve.—He has also found it useful in large doses, in chronic ulcers, and in a genuine case of traumatic tetanus, and would recommend its trial not only in tetanus, but also in hydrophobia, in doses of half an ounce every two hours.

Dr Elliotson extols *acupuncture* very highly, chiefly in rheuma-

tism of fleshy parts,—and the more so, as the disease is less inflammatory. "The effects are often magical. Of forty-two cases, taken in succession from the hospital books, thirty were cured."

Mr William Wallace, surgeon to the infirmary for diseases of the skin at Dublin, describes a peculiar eruption of a fungous character, to which he gives the name of "*morula*," from *morus* a mulberry, from its resemblance to this fruit. A minute description, for which we have no space, and a plate would be necessary to convey an idea of this disease. Mr W. has met with the complaint chiefly in the vendors of old clothes; it appears to be contagious,—is not of syphilitic origin, and is very readily cured by mercury, internally or externally employed.

Local applications, not containing mercury, have little influence over the disease.

The three succeeding papers relate to injuries of the hip joint, the diagnosis of which is so difficult, even to experienced surgeons, that every observation on the living or dead subject demands attention.

The first paper, by Mr George Langstaff, mainly consists of a description of various morbid preparations in his possession illustrating the kind of union which results after fracture of the cervix femoris within the capsule. Whether a bony union is ever possible has been denied by many surgeons; that it is a rare occurrence has been admitted by all. The facts stated by Mr L. are interesting and valuable, although he details no case in which a complete bony union had occurred. In one, however, the union was principally osseous and partly cartilaginous; the neck of the bone was greatly shortened by absorption; the capsular ligament was immensely thickened, and embraced the joint very closely; the cartilaginous covering of the head of the bone and acetabulum had suffered partial absorption; and the internal surface of the capsular ligament was coated with lymph. The patient was a woman about 50 years old, had been confined to bed nearly 12 months, and during the remainder of her life, which was ten years, walked on crutches.—In other cases, Mr L. discovered various alterations of structure indicative of the degree of power of reparation possessed by the injured parts. In all, the neck of the bone was shortened; in most, the union was effected by short ligamentous fibres,—sometimes there was a

partial joint; and in very many cases, the capsular ligament was greatly thickened, often covered internally by lymph or even cartilage, and in two cases at least, if we understand the description, portions of the capsular ligament became osseous, thus increasing the firmness of the injured parts. Mr LANGSTAFF has long been satisfied of the possibility of bony union in fractures within the capsule, but "why the ossific union of fractures within the capsule is of more rare occurrence than in fractures exterior to that ligament, is still a pathological problem of difficult solution." We, however, perceive no difficulty; for the upper fragment, when the fracture is above the insertion of the capsular ligament, is so sparingly supplied with nervous influence and blood (the round ligament and synovial membrane being almost the only media of communication) that it can contribute very little, if any thing, to the process of regeneration: in the language of Mr HUNTER, it merely "accepts of the union" offered by the inferior fragment.

The second paper, by Mr EDWARD STANLEY, of St Bartholomew's Hospital, records some cases of fracture of the trochanter and of the root of the cervix, in which the diagnosis was difficult, owing to the fragment of the trochanter being drawn to the situation occupied by the head of the os femoris in some dislocations at the hip joint. The difficulty is often increased by the fact that the foot in such accidents is occasionally turned inwards, as in dislocations, and crepitus is not always to be detected. Why the limb should ever be inverted in such cases is not easily to be determined; it probably depends on the manner in which the accident has occurred. Mr S. records, also, a case in which there was a fracture of the neck within the capsule without any rupture of the synovial and fibrous covering. The patient died, from an inflammation of the intestines, in the fifth week from the date of the accident. There was a slight effusion of blood under the membranes and in the tract of the fracture. Such cases would no doubt unite by bone.

In the third paper, on accidents of the hip joint, Dr BRULATOUR of Bourdeaux gives the particulars of an accident which occurred, on the 20th of March, to Dr JAMES, an English physician, at that place. Dr J. was thrown from his horse and manifested the principal signs of fracture of the neck of the femur. He was confined to his bed for two months; during the whole of which period, permanent extension was preserved. At the end of the third month,

his crutches were put aside, and he walked with only the assistance of a cane. In August, he could walk without any assistance, and subsequently recovered the full use of his limb. He, however, died in December from a violent attack of hæmatemesis.

On examination of the hip joint, there were observed, 1. The capsule a little thickened; 2. The cotyloid cavity sound; 3. The interarticular ligament in a natural state; 4. The neck of the femur shortened; 5. An unequal line surrounded the neck, denoting the direction of the fracture; 6. At the bottom of the head of the femur, and at the external and posterior part, considerable bony deposit had taken place. A section of the bone was made in a line drawn from the centre of the head of the femur to the bottom of the great trochanter, so as perfectly to expose the callus. The line of bone indicated by the callus denoted also that the bottom of the head of the femur had been broken at its superior and posterior part.

On paraplegia, by H. Earle, Esq. Surgeon to St Bartholomew's Hospital, &c. Nervous diseases are the most intricate and the most unmanageable affections to which the human system is subjected. Their pathology can only be revealed by investigations primarily illustrating the physiology of the brain and its dependences, and by a careful comparison of facts from the morbid anatomist. Unfortunately, in our opinion, the past and the present medical philosophers have regarded too exclusively the vascular system in nervous diseases,—these last being regarded as secondary, the result of sanguine irritation. Hence the discovery of vascular turgescence, of effusion, or of structural alteration, has been deemed by many an adequate explanation of the phenomena during life, and, of course, a sure basis for therapeutical measures; forgetting that these nervous complaints, in all their intensity, have often existed without any evidence, ante seu post mortem, of vascular irritation, are seldom in proportion to the degree of sanguine disturbance, and, moreover, that such vascular affections are very frequently detected without any corresponding nervous disease. But little examination of bodies will, we think, confirm these facts. The nervous system *should be studied by itself* physiologically and pathologically; for on it are impressions primarily made, and disorders originally induced: the active or passive disturbance, excited or depressed state of the circulation is very generally secondary, and although demanding much

attention is comparatively unimportant. These opinions we have long entertained, and they daily derive support from reading and observation. An analysis of the very interesting and valuable cases which Mr EARLE has presented in this paper would confirm, we believe, the above views, but we have no space for further remarks or even for a summary of his facts. We shall present a few extracts illustrative of the opinions which he has deduced from the cases and dissections presented.

By paraplegia he understands that species of palsy in which both sides of the body, partially or generally, are affected, in opposition to hemiplegia, in which the affection is confined to one side. Paraplegia is not alone dependent on disease of the spine, or its contents, but will occasionally be found in chronic diseases of the brain and its membranes. The diagnosis is not always easy, but may often be tolerably correct, if a close attention be paid to many circumstances.

"Paraplegia, dependent on disease in the brain, generally occurs at the middle or more advanced period of life than is usual in diseases of the bodies of the vertebræ, or their intervening fibro-cartilages. Its progress is more rapid than the slow insidious approach of symptoms from the latter diseases; the affection is more general, occasioning more or less paralysis of the upper and lower extremities, and this will often take place in a very few days from the occurrence of the complaint. The disease happens much more frequently in men than women. The gait of persons suffering from cerebral affection is peculiar, and very different from that attendant on affections of the spine. It very nearly resembles the vacillating steps of a drunkard.—Such paralytic persons are incapable of walking in a direct line; the limbs are loose, and thrown forward with an exertion of the whole body; there is a great consciousness of feebleness in walking, and the greatest difficulty in turning round. The appearance of the eyes often much resembles those of a drunkard, particularly when the patient is at all excited or anxious. The above analogy to the staggering steps of intoxication is readily understood, if we consider, that it is the temporary disturbance from the congestion of its blood vessels, that deprives the drunkard of the power of directing his steps, and for a time induces a state bearing the closest resemblance to paraplegia.

"Sensation is more impaired than in spinal affections, when it will often remain perfect after a total loss of the locomotive powers. This impaired sensation is often peculiar, imparting an idea of some foreign body, as a leather glove or stocking being interposed. The patient appears to feel, if I may use the expression, through a false medium; the limbs are more wasted and flabby, without any spasmodic rigidity of the muscles, which so often occurs in affections of the spine. Although often accompanied with a torpid state of the bowels, aggravated, no doubt, by the impaired muscular power of the abdominal parietes, there has not, in any instance I have witnessed, been any train of gastric symptoms, similar to those which so constantly attend affections of the spine, especially of

the dorsal region. In some instances, there is the additional confirmation of an impaired state of some of the external senses, accompanied with vertigo, a sense of weight on the head, and a general disturbance of the cerebral functions. As disease advances, the power of the brain in transmitting its influence to the extremities becomes more and more circumscribed.—Thus I have known a tubercular affection of the pia mater, in the first instance, cause a numbness and loss of feeling in the feet, which has gradually extended until all four extremities were completely paralysed, and the muscles concerned in respiration at length refusing their office, death ensued."

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"Whenever disease has proceeded so far, one or more of the mental faculties has, in almost every instance, suffered, and no reasonable doubt can, in such cases, be entertained respecting the real seat of the disease. It is in slighter and more chronic cases that it is often difficult to form a correct opinion; yet to establish a correct diagnosis in such cases is of the utmost importance, both with respect to the prognosis we should form of the probable termination of the case, and with reference to the proper treatment to be adopted, that we may not subject the individuals to useless suffering from the application of caustic issues and setons to the spine, and the disappointment which would follow their obvious inutility in such instances. In several of these cases, attention to the following circumstance has materially assisted me in forming a correct judgment; I am therefore induced to lay considerable stress upon it, although to some persons the opinion drawn from it may appear somewhat speculative. It is well known, that when a nerve is stimulated or injured in any part of its course, the painful sensation is referred, by the percipient mind, to the sentient extremity of the nerve; the familiar instance of the pain referred to the extremity of an amputated limb, may be adduced in proof of this. The exact reverse of this takes place, when there is a partial paralysis arising from morbid affection of the cerebral organs. Here the centre of the sensorial functions being impaired, it appears to be incapable of transmitting its influence to the extreme parts of the body, and thus the feet and hands gradually lose their sensation or power of motion, or both; and in such cases if the nerves supplying the limbs be irritated, *they will convey the impression of such injury only part of the distance down the limb, about as far as the commencement of the paralytic affection.* I have repeatedly examined cases of paraplegia from affection of the spine, and in no one instance have met with the same phenomenon, which I have therefore been induced to consider as diagnostic of the paralytic affection being dependent on disease of the brain or its membranes, which opinion has, in several instances, been confirmed by examinations after death, in which both brain and spinal cord have been carefully investigated."

Mr E. cautions us against a fruitful source of error, relative to the seat of the disease in paraplegia, arising from the incurvated state of the spine. This spinal curve in affections of the brain implicates the whole column, which resembles a half-hoop, in consequence of the debility of the muscles of the back, which are no longer capable of maintaining the erect posture. The curve will immediately and spontaneously disappear in a horizontal position. In diseases of

the spine, the curve is more angular and defined; and distressing symptoms often follow attempts at restoration.

This valuable communication is closed by a detail of cases—1. Of paraplegia dependent on cerebral affection alone; 2. Of paralysis from diseases of the spine; And lastly, where disease existed in the same individuals in both brain and spinal marrow. They are all worthy of record and examination.

Observations on the necessity and method of further investigating the distinctions between syphilis and other varieties of venereal disease, by R. Welbank, Esq.

This paper, manifesting some talent and observation, does not, however, admit of analysis without making very copious extracts, for which we have no room. The author would seem willing to refer the various primary and secondary affections from impure coitus to two or three distinct poisons, ascribing their innumerable varieties to the tissue concerned, and especially to the peculiarities of the patient's constitution. The two most important venereal affections are syphilis and phagedæna; the former generally curable by mercury and often demanding its employment, the latter seldom benefited by this mineral. To these he would add the venerola vulgaris of Mr Evans, seldom demanding active treatment.—To those who are investigating the varieties of syphilis, the paper of Mr Welbank will afford many useful hints and cases.

The concluding paper in the volume gives a detailed account of a successful case of amputation at the hip joint, executed by Mr John Orton under very unfavourable circumstances. The patient had been long suffering from caries and abscesses of the knee joint and thigh, some of the sinuses extending to the buttocks. He was excessively reduced by hectic fever, and was also suffering from a "bed-sore" on the sacrum with necrosis of this bone and the coccyx. The operation was performed June 10th, 1824. In August he was able to move about on crutches, and recovered completely, remaining well in January 1826. Much credit is due to his surgeon for the assiduous and scientific attention manifested in the after treatment, to which the unexpected recovery of the patient is to be mainly attributed. Any man with common dexterity and courage can remove a limb; few, however, can so combine science with art,—the constitutional with local treatment,—as to afford the patient

the best opportunity of escape, confer permanent honour on their profession, and facilitate the true interests of humanity.—In our estimation, Mr ORTON has gained more credit by his after treatment than even by his well executed operation.

We cannot dismiss the volume before us without a passing tribute to the lithographic plates executed with much neatness by J. PERRY.

Medical Literature.

ARTICLE XII.—*On the Reciprocal Circulation of Blood between the Mother and Fœtus. By* M. MERY.—*With Preliminary Remarks. By* JOHN REDMAN COXE, M.D. *Professor of Materia Medica in the University of Pennsylvania.*

IN the July number of the North American Medical and Surgical Journal, page 168, is a very interesting abstract of a paper of BIANCINI, "on the communication between the blood-vessels of the uterus and placenta." In this instance, the blood-vessels of a woman who died in child-bed were injected, *the placenta being undetached.* The injection, on dissection, was found to have passed *into the vessels of the chorion and amnios*, the tortuous arteries of the uterus having penetrated the tissue of the placenta, &c. Other analogous facts are adverted to, relating to the human, as well as to the brute subject. How the opponents of this opinion will overcome the dilemma in which the above fact places them, we know not; but presume, as heretofore, they will boldly deny the fact, or distrust its accuracy*.

* The demonstration of this fact so long ago as 1752, by the same means of injection, rests on the authority of the highly celebrated M. LE CAT. This communication was made to the Royal Academy of Sciences, Belles Lettres and Arts at Rouen, on the 11th of July of the above year. A slight notice of it appears in the Recueil Périodique d'Observations de Médecine, Chirurgie, Pharmacie, &c. 1754, Vol. I. p. 253, in the following words, "Le même (M. LE CAT) ayant démontré à l'Académie, le 11 Juillet 1752, la communication des vaisseaux sanguins du fœtus et de la mere, dans une placenta attaché à un utérus et injecté, il a trouvé cette année *deux* occasions pareilles de répéter cette expérience, la premiere fois devant les commissaires de l'Academie, et la seconde, en particulier, avec un succès égal a celui de 1752."

It must be more than simple denial, that shall be permitted to overthrow authority like this; or even to explain the experiments away by sophistry or plausible argumentation.

It would indeed seem, that attachment to hypothesis has always led to such uncandid measures; not on the point in question only, but on many others; hence too it is, that numerous and well attested facts in ancient writers have been completely overlooked or forgotten in the present day. It would seem as if all vestiges of *ancient* knowledge, either as to fact or theory, are now considered nugatory; at least they are rarely brought forward, unless they should chance to uphold some favourite opinion, probably advanced as new in medical science; for dogmas and hypotheses now sprout forth like mushrooms;—and fortunately, if rapid in their growth, they have, within the last few years, been equally so in decay. It has been said by a wise man that there is nothing new under the sun; certain it is, there are few of the *so esteemed new* doctrines in physiology, pathology or other parts of medical science, but can be clearly found to have formerly prevailed! If the publication of that cumbrous work, the "Dictionnaire des Sciences Médicales," in sixty volumes has been attended with the disadvantage of leading our writers (both *of* books and *in* journals) to quote largely from or to refer to authors whose works they have never seen and rarely heard of; it has so far been useful, that it has elicited the names and productions of many illustrious medical worthies, who may possibly be hereafter deemed worthy of recognition by our schools; and even recommended as teeming with useful and important truths. We speak of GALEN and HIPPOCRATES with apparent candour and approbation: we still call the latter the father of medicine, &c. And yet few think them worthy of the slightest personal acquaintance. In this respect, we are to blame; if great as they are acknowledged, they deserve to be investigated; and if they are not, the sooner this is demonstrated the better, that they may not be ever again quoted as authority.

What is above said may be considered as irrelevant to the object mentioned at the commencement of these remarks: and yet it is closely connected therewith, inasmuch as it is this total neglect of the older writings that has precluded us from knowing that facts as strong as those of BIANCINI have long ago been given to the world, in order to settle a question then agitated amongst physiologists with equal warmth as latterly.

In BERRYAT'S "Mémoires ou Collections de Piéces Académiques," Vol. II. p. 624, we have a communication from M. MERY to the Royal Academy of Sciences at Paris, in 1708, which I should have

supposed, would, *if known*, have been sufficient to settle the dispute, or that at least it would have been employed in the later discussions of the question. It is scarcely necessary to request the publication of the whole communication. The work referred to is so rarely to be met with, that I presume M. MERY's paper to the majority of readers, will prove in truth a *new* one; and I regard it as a favour to the profession to bring it to their knowledge. The problem, again verified by the experiments of BIANCINI, it will be seen, was ably and fully solved by M. MERY a hundred and twenty years ago; and by a different process, tending thereby to render the truth more impressive. The character of that gentleman, as an accurate observer and accomplished surgeon, must be known to many. It will be perceived that the question had been one of long standing in the schools, but we trust is now definitively settled. The second problem, which M. MERY also resolves by reference to the first, viz. the mode of nourishment of the fœtus, is not essential to the subject under consideration: it is therefore omitted.

M. MERY's communication is thus headed.

A Problem in Anatomy: to wit,—Whether, during pregnancy, there exists a reciprocal circulation of blood between the mother and child.

The opinions of anatomists, says M. MERY, are at present strongly divided on the question. On the one side it is affirmed that the blood of the mother *does not pass* into the fœtus, nor that of the fœtus into the mother; and that the only nourishment it receives from the mother is chyle furnished by the glands of the uterus.

On the other side they are persuaded, *that there is* a reciprocal circulation between the two; and that the infant is nourished only by the maternal blood, which is sent to it from the uterine arteries through the umbilical vein; and it is moreover denied that any glands exist in the uterus.

Previously to examining whether the womb of the mother has or has not glands, and whether the infant is nourished only with the chyle or with the blood of the mother; it is first necessary to be well assured, whether it be true or not, that there exists between them a reciprocal circulation of blood; because, this fact being ascertained, will enable us more easily to know with which of those

two aliments the woman furnishes the fœtus during its continuance in her womb. I shall begin therefore by examining if the blood circulates or not in each reciprocally. To effect this inquiry, I shall make use of an unfortunate event, which has opportunely happened for the absolute solution of this first problem.

A woman of thirty-five or forty years of age, forcibly detained at the general hospital, was conveyed some time since to the Hotel Dieu to lie in, being near her time, and in perfect health. This poor woman, fearing that after delivery she should be again confined, undertook to escape from the lying-in ward, which is very elevated. To execute her intention more secretly, she fastened during the night, to a window of the ward which overlooks the Rue de la Boucherie, a rope knotted at intervals, to descend more readily. But having taken her measures illy, the rope was found to be much too short, so that reaching the extremity, she was compelled to fall on the pavement below. In falling, she luxated one thigh, and broke the other, the bone protruding through the flesh, making therein a very extensive wound. In this unfortunate condition, she was carried back into the Hotel Dieu, and died in half an hour.

As soon as she was dead, one of the house surgeons opened the *belly*, which he *found filled with seven or eight pints of blood.* After removing this, he opened the uterus, in hopes of saving the infant, but perceived that it was already dead; and that *all the vessels of the woman were entirely empty of blood.* He likewise remarked that the body of the placenta, as well as the membranes, *was still united to all the internal surface of the womb;* which had prevented the blood of this woman and that of her fœtus from flowing into its cavity; as is the case when the placenta separates from the bottom of the uterus.

The union of these two parts, and so large an amount of blood discharged into the belly of this poor woman, caused me at the moment to make this reflection. The mother and child die at the same time from great loss of blood occasioned by the disunion of these parts. I have then, in order to discover if the blood circulates reciprocally from one to the other, only to examine whether the vessels of this child are void of blood, equally with those of its mother: for if they are full, it is certain that the blood of the fœtus does not pass into the vessels of the woman: but if, on the contrary, I find them empty, it is evident that a reciprocal circulation exists between them.

This idea strongly excited my curiosity, and to satisfy it, I immediately examined the little corpse. In this research I found not the slightest wound or alteration on any part of its body; and passing to the interior, all the parts appeared equally sound. Finally pursuing my examination, *I found the veins and arteries of this* fœtus *nearly, if not entirely, empty.*

Now, since the blood had not escaped into its abdomen, its breast, nor elsewhere; it is evident, that the vessels of the mother being opened by her fall, all the blood of the child had flowed with that of the mother into the cavity of the abdomen.

And, since the blood of the child could take no other route than that of the umbilical arteries and the uterine veins, in order to reach the belly of the mother; it follows consequently, that there existed during life, a reciprocal circulation between both. Nothing further then remains, but to explain the manner in which this was accomplished during the whole term of this woman's pregnancy. The facts to be stated will assuredly conduct us to this explanation.

I showed to the Royal Academy of Sciences, on the 23d of February 1706, that the uterus of a woman, who died four hours after delivery, had no glands; that its internal surface was without a membrane; that the placenta that was united with it had none on its exterior surface; that the vessels, which terminated at these two surfaces, were manifestly open, and that the substance of these two parts was fleshy and altogether spongy; and consequently enabled readily to imbibe the blood which their vessels diffused reciprocally from one to the other.

These facts being verified in more than fifty other women on whom I have performed the cæsarian section after death; and being assured that the blood of the infant of whom I have spoken was not able to escape into the belly of its mother except by taking the route of the veins of the uterus open on its interior surface to receive it, it is easy to perceive that the blood of its arteries, also open on the same part, must, during the whole period of this infant remaining in the womb of the mother, have passed into the branches of the umbilical vein, equally open on the exterior surface of the placenta, to give it a passage by the channel of this vein into the body of the infant, in order to fill the vessels which were constantly discharging into those of the mother. The four following remarks, prove still more fully this circular movement of the blood.

In the first place, we know that, after delivery, the placenta and

its membranes being separated from the fundus of the womb, the blood that flows from the arteries of this part of the woman, not being able to enter the veins, spreads through its cavity and is discharged by the vagina. It cannot enter the veins, because their openings being on the internal surface, they cannot communicate with those of the arteries there terminating. When then the placenta is united to the womb, the blood which flows from its arteries ought to enter into the roots of the veins of the placenta, whilst that which flows from the umbilical arteries takes the route of the veins of the womb.

In the second place, we also know that if, during pregnancy, the placenta separates from the fundus of the womb before the woman is in labour, both mother and child perish, their vessels exhausted of blood, *how short soever the time of the discharge be**. This exhaustion could not take place if the surfaces by which the womb and the placenta are united, were covered with membranes; and if it were true that the blood of the arteries of the uterus (open on its interior surface) passed during pregnancy into its veins; and that that which is carried by the branches of the umbilical arteries to the exterior surface of the placenta, entered into the roots of the umbilical vein. Nevertheless the mother and child die, their vessels emptied by the separation of the placenta, although the woman does not fall in labour. It is evident therefore that the branches of the arteries of the uterus, which terminate on its interior surface, do not unite with the roots of the veins which there originate. It is equally the case with the vessels of the placenta. Hence, whilst the placenta remains in union with the uterus, the arteries of that ought to spread their blood in the spongy substance of the placenta, and the umbilical arteries discharge their blood into the porous substance of the womb, to be taken from thence by the veins. It is then certainly true, that between the woman and her fœtus there is a reciprocal circulation of blood. It is obviously on this account, that the surfaces by which these two parts are connected have no membranes, and that their substance is altogether spongy—hence, when

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* This assertion of M. Mery must be regarded as of a general character, and as applicable to the period at which he wrote, rather than to the present, since numerous cases of flooding from the cause he mentions are recorded, in which the lives of both parent and child have been preserved, by recourse to immediate delivery;—it has however no bearing on the main point.

separated, by pressing either of them, the blood flows from the surfaces that are not covered with membranes, but cannot escape from those parts that are covered.

Thirdly,—When, on the contrary, the placenta being still united to the uterus, a woman dies in labour and her fœtus perishes at the same time by compression of the umbilical cord, then the vessels of the mother and child are equally filled with blood. The fœtal cord was free in the uterus of the female whose tragic history I have detailed; and both being dead, the veins and arteries of both were found empty; because the vessels of the mother being broken by her fall, all the blood of her infant had flowed off, together with her own, into the cavity of her abdomen. These two events, connected, evidently prove that there is between the mother and the child a reciprocal circulation of blood.

Fourthly,—If the blood of the umbilical arteries do not pass into the veins of the uterus, nor that of the arteries of the uterus into the veins of the placenta, the respiration of the mother ought to be absolutely useless in maintaining the circulation in the body of the child. If so, it is essential that the fœtus should have in itself all that is necessary to it, for the circulation of its own blood in its vessels; it might then live after the death of the mother *equally long, in the uterus*, without receiving any nourishment, as it could do, *out of that cavity*, without aliment. The reverse of all this occurs, the infant perishes as soon as the mother ceases to breathe, or if the umbilical cord of the fœtus suffers too great compression during her life. We must then acknowledge that there is between the two a reciprocal circulation of *air** and blood, and that the infant has not, *per se*, so long as it is retained in the uterus, the first principle which affords motion to its blood. The respiration of the mother is therefore the first cause of the fœtal circulation, since it perishes as soon as she ceases to breathe; and hence I conclude that an opposite opinion has every appearance of being incorrect.

Finally,—If it is true that nature acts always uniformly in the same operations, as we have reason to believe; the same should be the case in all viviparous animals, viz. that a reciprocal circulation should exist between them and the fœtus.

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* The erroneous opinion here manifest, of a reciprocal circulation of air, does not invalidate the force of the argument so ably employed by M. Mery.

ARTICLE XIII.—*Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus, Guilielmi Harveii, Angli, Medici Regii, et Professoris Anatomiæ in Collegio Medicorum Londinensi.—Apud Harveii Opera omnia. Lond.* 1766. 4*to.*

We propose to lay before our readers a few remarks and extracts from the celebrated essay of HARVEY on the Circulation; premising an expression of regret at the meagreness of this article, which however we are compelled to submit to by the crowded state of our department of original communications. Indeed it is hardly befitting so great a subject to treat of it in a short paper; for although the essay itself is a short one, and will be found on examination not to have occasioned the development of any immediate and important changes in the medical practice of the time, yet in its remote consequences it may be justly regarded as the pivot on which most of the successive improvements have turned.

A proper examination of HARVEY's work could in fact only be effected in a considerable volume. Such a volume ought to present a clear detail of the principles of medicine as they were maintained anteriorly to his era; a judicious exhibition of anatomical, physiological, and therapeutical science as they existed in the schools, hospitals, and private ministrations of eminent men during his lifetime; an analysis of the work itself; and a history of its progress, contests, and final reception and triumph, over all Europe. In this way a vast fund of interesting anecdote and useful information might be accumulated; and if it could be closed by a history of the progress of *solidism* down to the present period, it would certainly constitute a most valuable addition to the medical library: for, since the most of modern pathology is embraced in the modifications of the living blood vessels, such a work would furnish a concentrated view of the principal laws of that department of medicine. It seems scarcely necessary to say that we have neither leisure, space, nor materials for such an undertaking. Such a task could not be performed in the United States in the present condition of our medical libraries.

WILLIAM HARVEY, the discoverer of the circulation, was born at Folkstone, in Kent, England, on the 1st of April 1578. After spending six years at Cambridge, where he was admitted in 1603, he proceeded to Italy, in order to study medicine under the celebrated masters who then taught at Padua. JEROME FABRIZIO, call-

ed from his birth-place FABRICIUS AB AQUA-PENDENTE, gave lectures and demonstrations in anatomy: THOMAS MINADOUS and JULIUS CASSERIUS both taught in the same school; the former, practical medicine; and the latter, surgery.

FABRICIUS was the successor of FALLOPIUS, who himself had been preceded by the great VESALIUS; so that HARVEY expected, naturally, to enjoy the highest advantages which the then modern anatomy might bestow, in a school illustrated by such great names. Human dissections were not rare, and the demonstrations, made from the actual subject, and illustrated by frequent exhibitions of comparative anatomy, were thought to be very perfect and satisfactory. Some advances had certainly been made in the knowledge of particular structures; but the great points of the science were still handled in the Galenical manner, no man daring to differ ever so slightly from that great master, without much precaution, abjuring of all intentions to fly in the face of authority, and free confession of his paramount claim to confidence and faith. Notwithstanding all the advantages of this great school, HARVEY enjoyed fewer lights than the humblest student of the present day; for at that period no lymphatic, lacteal, capillary or arterial office was even suspected, if we except what ASELLIUS had written on the lacteals, and which seemed to have been forgotten or despised. The heart's true motion and function were unknown, and the vital functions, misunderstood *ab ovo*, were misconceived of *usque ad mala*.

It is very common, when any thing is said about the older writers, to laugh to scorn their whole system of physiology, pathology and therapeutics—and it would seem at first view, that they must have been subjected to the most deplorable embarrassments in the theory of medicine, by their ignorance of the circulation, of absorption, and the nervous functions. On the knowledge of the first of these functions are mainly dependent all the principles of modern pathology; and since we treat as impostors all those who undertake to manage the sick, being ignorant of this *abecedarium* of medicine, what respect, it is asked, can we have for a pragmatical set of old pretenders who were no better than quacks.

But this is a great mistake. Those old physicians were men of consummate skill: their histories of diseases have not been and probably cannot be improved; their prognostics excite our astonishment; and the success of their practice was often rewarded by the confidence of a whole people. The diagnostics of GALEN in the

case of MARCUS AURELIUS, and the conduct of PARE in that of the lord of Martigues, would be a subject of pride even to such men as COOPER or BROUSSAIS. The fact is they did better, by profound investigations of the tendencies, stages, and external characters of diseases, what we do worse, by our ready principles, which, anticipating personal experience and observation, seem to do away the necessity of severe labour :—their works, which contain their methods of thinking, may teach us how to learn to think :—a method, which, with all their disadvantages, led them to happy results is at least worthy of our most careful study.

HARVEY learned at Padua, that the first product of digestion (the chyle) was attracted (absorbed) from the intestinal surfaces by the mesenteric veins, which, converging towards the liver, delivered it into that organ, where by virtue of a certain faculty it was converted to blood,—this conversion of chyle into blood, or hæmatosis, was effected by the influence of the ipsa epatis caro ; a specific power of conversion, or facultas alteratrix, residing in every organ or system of the body, brought about these mutations,—the liver alone was the seat of sanguification and the source of all the veins of the body. Blood therefore, manufactured in the organ which gave origin to all the veins, was conveniently sent out by those veins to the parts—the arteries and heart had nothing to do in the matter of circulation. He was taught that the blood moved up and down in the veins according to the necessities of any particular case, without having any fear of the valves before its eyes, and without regard to any other of the manifest impossibilities of such an absurd ebb and flow *ad instar Euripi*.

Some supposed it to flow to the parts they knew not how ; but others were satisfied that it got to the parts by virtue of an attraction exactly like that which M. BROUSSAIS and his friends suppose to operate at the present day in all cases of vascular irritation, i. e. the parts which wanted a supply had only to dilate themselves just as the thorax is dilated by the act of breathing, and the blood immediately rushed towards the vacuum, impelled by the double power of venous contraction from behind, and attraction, suction or dilatation from before it. When the blood had reached the *parts*, it was dismissed from the care of the vessels and delivered over to the *intervascular spaces*, where it became gradually converted into flesh, membrane, &c. &c. as the case might be, but never returned to the

heart again ; it was only formed in proportion to the necessities of nutrition.

The whole of this doctrine is clearly explained by FERNELIUS at page 248 of Vol. I. of his works, and it strikes us as not a little remarkable that this celebrated Frenchman should have held in 1540, when he was totally ignorant of the circulation, a doctrine as to attraction of fluids by irritation, and inter-vascular spaces or parenchyma, so exactly similar to that of his celebrated countryman of the physiological school. For it will be at once perceived that he and M. B. are quite agreed on these two points of doctrine. GALEN had settled it as a proven fact, that blood is not naturally contained in the arteries, for they always were found empty after death. The hemorrhages from arteries occasioned by wounds never convinced the profession that this idea was false ; the emptiness of those vessels could only be accounted for on the supposition of their having during life contained an intangible and imponderable fluid called vital spirits, the fruit of an union between air sucked into the lungs, changed by their organic sensibility, as above said, and conveyed to the left side of the heart by the pulmonary veins, and blood which transuded through the septa of the heart from the right to the left side. These vital spirits conveyed heat and life to the parts into which they were driven.

It is true that some modification of this doctrine had been effected by the discoveries of SERVETUS and COLUMBUS, viz. that the blood of the right ventricle passed by the pulmonary artery to the pulmonary veins and so on to the left side of the heart ; but still, this glimpse of real anatomical physiology altered no body's opinion concerning the uses of the lesser circulation, the blood of which was still supposed to get to the left ventricle only for the purpose of compounding the vital spirits. Even the knowledge of the valvular structure of the veins, which had been known long before FABRICIUS's time, never prevailed on them to give up the old doctrine of a progressive motion of the blood from the larger trunks of veins to the smaller, in what we now know to be a retrograde and impossible direction ; this too, while the argument of the valvular structure of the heart was triumphantly used to establish the lesser or pulmonary circulation.

As we do not intend to go into an investigation of the state of the anatomical and physiological knowledge of that period, we will satisfy

ourselves with repeating that HARVEY found the science when it was destitute of any knowledge of absorption, of circulation, or of the functions of the brain.

Having returned to London, he was in 1615 elected to be a lecturer on anatomy to the College of Physicians. He began his course in 1616, and *then* unfolded his unheard of doctrines. The original manuscript of these lectures, still preserved in the archives of the College, is entitled "Prælectiones Anatomiæ Universalis, per me Guilielmum Harveium, Med. Londinens. Anatomiæ et Chirurg. Professorem, A.D. 1516, prælect. April. 16, 17, 18."

These prælectiones were the first beams of that bright day of medical knowledge which their publication shed over the nations. Having laboured earnestly on the subject, which there is reason to believe he brought to a conclusion about 1623, he sent it to press in the city of Francfort in Germany, where it appeared, not however until the year of our Lord 1628, under the title of "Exercitatio Anatomica de Motu Cordis et Circulatione Sanguinis in Animalibus."

The book is dedicated to his master, king Charles the First.

He has given a proemium or preface, in which, having demonstrated the falsity of preceding opinions concerning the motions of the heart and blood and the functions of the arteries, he proceeds to the main object of his labours in Chapter I.

"When by the dissection of living and dead animals, and not by reference to the writings of others, I applied myself to investigate the motions and functions of the heart, I found the task so arduous, and full of difficulties, that I almost became convinced the motion of the heart could only be known to God. I could neither understand *how* the systole or diastole was effected, nor *when* or *where* the dilatation or constriction took place; because of the celerity of the motion, which in many animals occurred in the twinkling of an eye, or like a flash of lightning. Sometimes I thought I could perceive the systole, sometimes the diastole, at one while the motions were reversed, and at another utterly confused; so that my mind fluctuated. I could neither decide for myself nor receive the opinions of others, and did not wonder at ANDREW LAURENTIUS's assertion that the heart's motion resembled the flux and reflux of Euripus as described by Aristotle. At length, by means of greater diligence in the examination of many animals, and the collation of different observations, I thought I had mastered the subject, and extricated myself from this labyrinth of doubts by a discovery of the motion and use of the heart and arteries as I had desired.

"From this time I feared not to impart my opinions privately to my friends, and even publicly, in academic form, in my lectures on anatomy."

"Some were well pleased, others not; and these latter began to confute and calumniate me, making it out a monstrous thing that I had departed from the faith and doctrines of all preceding anatomists. The former, when they saw it new,

worthy of more inquiry, and likely to be of great utility, begged to have it more fully explained to them.

"Being at length moved by the entreaty of my friends to make them participators of the fruits of my labours; and partly induced by the envy of others who, receiving my words with an evil temper, and not understanding them well, endeavoured to traduce me with the public; and, in order that every body might be enabled to judge both me and my doctrine, I was induced to commit these pages to the press."

He mentions a further inducement to their publication, viz. that his learned preceptor FABRICIUS had omitted to speak of the heart in his works.

The second chapter contains remarks on the appearances presented by the heart during the vivisection of animals. He asserts that the *living* organ is observed to exist in two different states: one of rest or inaction, the other of action or motion; which succeed each other in a regular manner.

During the state of rest it is flaccid, as we feel it in death; it is relaxed, or enervated. In its state of motion three things are observable; viz. 1. It becomes erect or rigid, and strikes against the breast with its apex so as to be felt on the outside; 2. It is contracted in all its dimensions; 3. It becomes harder, as is manifest to those who take hold of it during the motion, and assumes a whiter colour in consequence of the expression of its blood, which returning to it during the relaxed state gives its sanguineous purple hue back again.

He declares the tension of the heart, the erection of its apex, the stroke against the ribs, and the pulsation of the arteries in the *whole* body, to be dependent on its state of action, not of rest. Hence the erroneousness of the common opinion, viz. that the heart was filled with blood at the moment of its stroke against the ribs, whereas the heart is emptied and not filled by the action.

In Chapter V. he shows that the auricles both contract at the same moment, and force their blood into the ventricles, which being filled, the heart immediately erects itself, contracts the ventricles and throws the blood simultaneously into the two arteries, the pulmonary on the right and the aorta on the left. Such is the harmony or rhythm of these motions, that the four appear to be only two. After this he takes up the proofs of the lesser or pulmonary circulation which had before been announced to exist by COLUMBUS and SERVETUS; but still the function was not well understood, the nutritious office of the pulmonary artery not yet having been abandoned. HARVEY's arguments are drawn from the fact that if the septum of

the heart were to be removed no obstruction would exist against the flow of blood down the aorta; from the foramen ovale and ductus arteriosus of the fœtus; from the structure of animals with a single heart, &c. "In perfectioribus nimirum et calidioribus animalibus, iisque adultis (ut in homine) sanguinem de dextro ventriculo cordis per venam arteriosam in pulmones, et inde per arteriam venosam in sinistram auriculam, et inde in ventriculum cordis sinistrum, permeare contendo: et primum posse hoc fieri; deinde ita factum esse."

In the eighth chapter he proceeds to establish his immortal doctrine of the great or aorto-cavic circulation, and evidently trembled on the verge of that precipice down which he dared to hurl the whole structure of the ancient physiology of the vascular system.

"When," says he, "I shall have spoken what it remains for me to say concerning the quantity and abundance of blood that passes through the heart, it will be so new and unheard of a proposition, that I have to fear not only evil from the envy of some men, but that I shall make all men my enemies; so much doth custom, deep rooted prejudice in favour of a doctrine once imbibed, and a venerable regard for established opinions, weigh with mankind: but be this as it may, the *die is cast*, and my hopes repose on the candour and love of truth of the learned. Truly, when I often reflected on the great quantity of blood I had observed in my experiments and investigations in living animals, and in opening the arteries, on the symmetry of the ventricles, and size of the veins and arteries which arrive at and depart from the heart; when I considered that nature who makes nothing in vain would not have given such a proportion to these vessels for nothing; when I observed carefully the structure of the valves, fibres, and other parts of the heart, and many other matters; I began to ask myself what quantity of blood is transmitted, in what time is the transmission effected, and whether the juice of our ingested aliment was capable of furnishing this quantity of blood for transmission. I reflected that by such transmission the veins would become completely exhausted and empty, while the arteries, on the other hand, would become filled and ruptured by the intrusion of such a quantity of fluid, *unless they could relieve themselves by pouring it into some other place, and finally into the veins by which it could return to the right side of the heart.* I began to reflect whether it might not have a *movement as in a circle*, and afterwards found this reflection just, and that the blood is protruded and impelled from the heart through the arteries into the whole body by the pulse (contraction) of the left ventricle of the heart (as happens in the lungs from the right to the left side through the pulmonary artery), thence by the veins to the vena cava and to the right auricle, just as occurs in the motion of the blood through the lungs."

We conceive it impossible to find any where a specimen of analysis more complete than that which HARVEY effected in studying this important subject. He did not fall upon his discovery by chance. Having examined separately all the materials, he then by

a synthetic process conducted according to the best plan and marked by a perfect candour, arranged its various parts, and presented to mankind the great truths of the circulation with a clearness, justness, and perfection which has left little or nothing for posterity to do. Dr WILLIAM HUNTER seemed little disposed to wonder at and admire this great discovery, thinking it not so strange that HARVEY effected it, as that it remained so long uneffected. But if the blessings it has conferred on mankind are inestimable, then we should hold him in the highest esteem; as he procured them and gave them to us, not by chance or by stupid good luck, but by a philosophical and laborious application of his vast intellectual powers trained and drilled to the very purpose. Truly the immortality of HARVEY, like that of CADMUS, COLUMBUS, and ARKWRIGHT, was well earned, and ought not to be subject to any questioning.

Our limits in this department being unusually narrow, we are compelled to omit the notice of the subsequent chapters of the treatise; but it seems hardly necessary to follow him closely through his arguments, which, at great length, are deduced from a multitude of facts that place the doctrine on its irrefragable basis.

HARVEY supposed the extremities of the veins to be endued with the absorbing power, for he appears not to have even heard of ASELLIUS's discovery of the lacteal vessels, which had been published in 1622.

It may be easily imagined that so signal a discovery would not fail to occasion much discussion in a profession so much given to disputing as ours—and we are sorry that the common complaint of "too many Medical Journals" could not have been made at that day, or we should have had reviews, cases and quarterly summaries of *circulating* matter. Poor as the press was in that day, a considerable number of works were published, either modifying, advocating or rejecting the doctrine. HARVEY himself did not escape the fate he seemed to fear; for if not all, yet most men became his enemies, and he lost his reputation and practice for what ought to have admitted him into the calendar at least, if not to some higher honour. He was pointed at as the *circulating man.* Nevertheless as he lived to an advanced age, he outlived all opposition, and found his doctrine received all over Europe. The immediate results of the discovery, as affecting the practice of physic, are not very evident. It is probable that the chemical doctrines of that day had much to do in retarding the beneficial tendencies of a clear view of the

circulation. Salts, sulphur, and mercury, fermentations, concoctions, &c. &c. stood mightily in the way of a dispassionate contemplation of the influence of vascular action on the vital phenomena. It could not be, however, that the same obscure notions should exist in regard to the uses and objects of blood-letting, which, divested of the absurdities of the galenical physiology, could now be practised on good principles.

After PECQUET, in 1645, had shown the course of the chyle through the lactiferous tubes of ASELLI into the thoracic duct, and thence into the stream of the circulation; and when BARTHOLINE and RUDBECK had described, in 1651, the lymphatics as far as they could discover them, nothing seemed to hinder the progress of the science, and it made daily acquisitions through the labours of GLISSON, BOERHAAVE, WHYTT, and HALLER, until its vast form having been at last blocked out by the rough hands of JOHN HUNTER, received the polish and elegance of a finished work by the masterly skill of BICHAT.

After all, we owe to HARVEY whatever we possess of true anatomy, physiology, or therapeutics; for neither one nor the other of these departments could have ever existed in their present perfected state, but for the happy direction and philosophic employment of his admirable genius and industry.

We take leave to repeat our regrets that some able pen has not been employed to pourtray all the relations and dependencies of our science on the labours of HARVEY. A strict and just comparison of the modes of practice, and its results, before and after the publication of his work; an examination of the causes which retarded in some degree the development of its influence; or even a modernized paraphrase of the doctrine it inculcates, would have a salutary tendency in diffusing a better taste, and at the same time expose a most excellent body of doctrine on all that portion of pathology which is conversant with the morbid modifications of our vascular system.

Bibliographical Notices.

ARTICLE XIV.—*Elements of Chemistry, including the Recent Discoveries and Doctrines of the Science. By* EDWARD TURNER, M.D. F.R.S.E. *Fellow of the Royal College of Physicians, and Lecturer on Chemistry, Edinburgh, &c. Edinburgh*, 1827. 8vo, *pp.* 723.

DR TURNER's work is divided into four parts: I. Imponderable substances. II. Inorganic chemistry. III. Organic chemistry. IV. Analytical chemistry. The second part, on inorganic chemistry, is treated of under four divisions. The first division comprises affinity, the laws of combination, the atomic theory and theory of volumes founded thereon, and the description of the simple non-metallic bodies, together with their compounds with oxygen, chlorine, iodine and fluorine. The second division treats of the remaining combinations between the non-metallic bodies, under the title of "compounds of the simple non-metallic acidifiable combustibles with each other." The third division is occupied with the metals, and the fourth, with salts.

This arrangement would seem to be imperfect. After giving Part I. for the imponderable substances, we naturally look in Part II. for the correlative title of ponderable substances, which admit very readily of being considered under the two heads of simple ponderable substances and their inorganic combinations, and organic compounds, corresponding to Dr TURNER's parts of "inorganic chemistry" and "organic chemistry." But waiving this objection, it was to be expected that some definite title would have been given to the first division of inorganic chemistry, as is done in respect to the other three divisions. This defect in arrangement is the more prominent, inasmuch as the title of the second division, by its restricted tenour, clearly implies that certain non-metallic combinations, selected on some principle of classification, have been already treated of in the first division. Besides, the subjects of affinity, laws of combination, atomic theory, do not harmonize well with descriptions of the non-metallic bodies, as treated of in successive sections of the same division of a scientific arrangement. Our objections would have been removed, if the author had treated "inorganic chemistry" under five heads, which might have been called, 1. "Philosophy of chemical action," embracing all that is said under the titles of affinity, laws of combination, atomic theory, theory of volumes, &c. 2. Simple non-metallic bodies, and their combinations with oxygen, chlorine, iodine and fluorine. 3. Remaining combinations of non-metallic bodies with one another. 4. Metals. 5. Salts.

Dr TURNER's arrangement here has some points of resemblance to that of Dr

HENRY. The difference consists in this, that while Dr TURNER treats of the whole of the non-metallic bodies, and their combinations with oxygen, chlorine, iodine and fluorine in one class, Dr HENRY treats of the same bodies and compounds, under two divisions; first, oxygen, chlorine, iodine and fluorine, denominated electro-negative bodies, and their combinations with each other, and secondly the remaining non-metallic bodies, and their compounds with the electro-negative bodies. Precisely the same bodies and compounds are treated of in Dr TURNER's first division, and in Dr HENRY's first and second divisions; but Dr TURNER's arrangement has the advantage of enabling a writer to treat of the non-metallic bodies in the most eligible order for teaching, without reference to an artificial arrangement, founded on the electrical relations of the bodies themselves. Thus Dr HENRY, after treating of oxygen, is forced to take up in immediate succession, chlorine, iodine and fluorine; while Dr TURNER is enabled to adopt the better teaching order, of oxygen, hydrogen, nitrogen, &c.

We hold it that a strictly scientific arrangement, and a teaching arrangement are quite different things, and that a treatise on any subject of science for a learner must be arranged in a different manner from one intended for a proficient. In a science like chemistry, in which, from the mutual dependence of facts, the chain of knowledge may be aptly compared to a circle without beginning or end, we have no other alternative than to commence with those substances, which possess the most diversified relations with other substances, and the knowledge of which may be most available in our progress. Now these advantages are secured in chemistry by beginning with oxygen, hydrogen and nitrogen, whereby the learner is made acquainted in the outset with water and the atmosphere. A knowledge of the mineral acids, and the alkalies is the next most important acquisition in the beginning; and the elementary bodies which they contain should be brought forward as early as possible, consistently with a proper regard to some arrangement as founded on the analogy of the bodies in properties. Nitric acid comes to be treated of early in Dr TURNER's arrangement by reason of the identity of its constituents with those of atmospheric air. Sulphuric acid, as a chemical agent, is even more important than the nitric; and though it might be difficult to give it an earlier notice than nitric acid, still we think it ought not to be postponed one moment longer than the necessity of the case imposes. Accordingly we should advocate the consideration of sulphur immediately after nitrogen, and chlorine next, with the view of adding muriatic acid to the stock of known compounds. After this, the remaining non-metallic bodies might be treated of in the following order:—Carbon, phosphorus, iodine, boron, selenium, fluorine, without regard to their analogies, but in the order of their relative importance.

Without disregarding entirely the object of having a class of metals, it is impossible to introduce to the learner a knowledge of the fixed alkalies and their radicals, at as early a period as their importance would render desirable. Hence it is, that in all our chemical systems we find potassa and potassium mentioned incidentally under nitric acid, and soda and sodium under muriatic acid,—anticipations rendered necessary in describing the processes for obtaining those acids.

These then are a few of the many difficulties which beset the subject of chemical arrangement; and that these difficulties have not been removed ought not to excite surprise, in a science, the objects of which are so multitudinous, and which is undergoing almost daily changes, by the contribution of new facts and observations.

After completing the account of the imponderable substances, Dr TURNER goes on to consider the subject of affinity. After this he treats of the laws of combination, the atomic theory, and the theory of volumes. The author deserves much praise for the able manner in which he has handled the latter subjects; and after reading the section devoted to them with attention, we can truly say that we have seen them discussed no where else in so successful a manner.

After this general expression of our satisfaction with this part of the author's work, we may be permitted to make some comments; and if we point out some slight defects in his reasonings and doctrine, we hope not to be charged with the desire of condemning in detail, what we commend as a whole.

At page 120—121, the author after having illustrated the law of multiple proportions by examples, in which it is shown that the different proportions in which one body combines with another have some simple relation to each other, as 1 to 2, or if there be more than two proportions, as 1, 2, 3, or 1, 2, 4, &c., he goes on to announce what he calls the general law in the following terms:—"When two substances, A and B, unite chemically, the quantities of the two bodies must either be equal, or in the ratio of multiples or sub-multiples of each other."

The law of multiples, as here given, is, to say the least, every obscurely expressed. It would seem to imply some peculiar relation in the quantity of one body to the quantity of another body with which it may combine, instead of expressing the relation of the different quantities in which the same body combines with another body. What is meant by the expression that "the quantities of the two bodies must either be equal," &c., is not easy to imagine, unless it is intended to convey the idea that the bodies must unite either in the primary proportion, or in some multiple or sub-multiple of the numbers, denoting such proportion.

At page 138, Dr TURNER says, "the theory of volumes has very considerable analogy to Mr DALTON's law of multiple proportions. The former is indeed, to a certain extent, a consequence of the latter; for if one body unites with another in several proportions, the quantities of the variable ingredient will stand in the same relation to one another, when expressed by volume, as they do by weight. But there is one remarkable difference. The weights of the two elements of a compound have no apparent dependence on one another. Thus, 6 carbon and 8 oxygen form carbonic oxide; 8 oxygen and 14 nitrogen form nitrous oxide; 8 is no multiple by any whole number of 6, nor 14 of 8. But the elements of a compound are always united by volume in the ratio of 1 to 1, 1 to 2, 1 to 3, and so on."

In the above quotation there are some truths, expressed in a somewhat loose manner. In the first sentence, the theory of volumes, which certainly embraces, not only the simple relation in which *different* gases combine in bulk, but also the simple relation of the successive proportions in which the *same* gas combines with some other gas, is compared with one branch only of the theory of combining weights, namely multiple proportions. It would have been more correct to say that *multiple* proportions by volume, using this expression in its ordinary acceptation as relating to different portions of the same body, follow as a necessary consequence from *multiple* proportions by weight, and for the reason which is so clearly expressed in the second sentence of the quotation. We may even go farther, and say, that *definite* proportions by volume follow as a corollary from *definite* proportions by weight; but how far the ratio of the numbers may be simple which

express these definite proportions in volumes, can be determined only by observation, and must depend upon the specific gravity of the different gases; that is, on the relation of the spaces which combining quantities of the gases by weight may happen to occupy. It is indeed true that the relation of these spaces, or what we may call the combining volumes of the gases, is exceedingly simple; but this by no means proves any remarkable *difference* between the *theory* of volumes and Mr DALTON's law of *multiple* proportions. It indeed shows that there is a difference between *combining volumes* and *combining weights*, in the simplicity of the ratios of the numbers which express them; for while combining weights are expressed by numbers which are not even multiples of each other, combining volumes are as 1 to 1, 1 to 2, 1 to 3, &c.

Though the numbers denoting combining weights are not multiples by a whole number of each other, they possess the curious property of being multiples by a whole number of the combining weight of hydrogen, which is that element which is found to combine in the smallest proportion. This observation was first made by Dr PROUT, who proved it to hold good in a number of instances. It was afterwards extended, and found to apply to a great many substances, by Dr THOMSON in his "First Principles of Chemistry," a work which we reviewed in our first Volume.

It is a remarkable fact that the chemists of the European continent do not seem willing to admit the above law of Dr PROUT; at least they are not found to adopt it in practice. After some study we have convinced ourselves that it is well founded. It is indeed true, that this law may be alleged to be altogether a fallacy, since in the higher atomic numbers on the hydrogen unit, the greatest deviation from an even multiple, (five tenths) will form but a small proportion of the entire atomic number, and may be considered within the range of the unavoidable errors of experiment. For this reason the higher atomic numbers cannot be appealed to with confidence in support of Dr PROUT's law. But the same objection does not apply to the smaller atomic numbers; and it is on that account that we have formed the following table, which embraces all those simple bodies, the atomic weights of which are under 21; the atomic weight being given, in the first column, according to the experimental numbers adopted by BERZELIUS and THENARD, and in the second, according to the latest determination of the British chemists, who generally admit Dr PROUT's law. The numbers are brought to the same standard, on the assumption that the atomic weight of oxygen is 8.

	Berzelius and Thenard.	*British Chemists.*
Hydrogen	.994	1.
Carbon	6.12	6.
Boron	8.60	8.
Silicon	7.40	8.
Aluminum	9.12	10.
Lithium	10.22	10.
Phosphorus	12.55	12.
Magnesium	11.77	12.
Nitrogen	14.16	14.
Sulphur	16.09	16.
Glucinum	17.66	18.
Calucim	20.48	20.

The nearest coincidences in the above table with Dr PROUT's law, occur in relation to hydrogen, carbon, lithium, magnesium, nitrogen, and sulphur; while the greatest discrepancies relate to boron, silicon, aluminum, phosphorus, glucinum, and calcium. If it be asked where would be the nearest coincidences with this law, supposing it founded in fact, the answer is, they would obtain in regard to those substances which are most important, and the combinations of which have been most carefully studied. Now the substances which answer to this description in the above table are hydrogen, carbon, nitrogen, and sulphur, and here we have the nearest coincidences with Dr PROUT's law. The greatest discrepancies are exhibited in the cases of boron, silicon, aluminum, and phosphorus, and these are among those chemical substances which have caused to chemists the greatest perplexity in determining their atomic weights. Hence it is fair to infer that the experimental numbers for these are more likely to be inaccurate, than those for the other substances included in the table.

If Dr PROUT's law be not well founded, how are we to explain the fact that hydrogen, carbon, oxygen, nitrogen, and sulphur combine so nearly in the ratios of the numbers 1, 6, 8, 14 and 16? If the combining proportions were not regulated by some law of this kind, would we not have, for the four last named substances, atomic numbers which would not contain the atomic number for hydrogen an even number of times without a remainder? We ask then why this close approximation to whole numbers, if it depend not on a simple relation between the weights of the atoms amongst themselves?

We have been drawn into a digression on this abstruse and curious subject, under the influence of a sentiment, that Dr TURNER has not dwelt sufficiently on Dr PROUT's law; and because we are convinced that the chemists of continental Europe have not paid that attention to it which its importance merits. That part of the theory of volumes of GAY-LUSSAC which alleges that different gases combine in bulks which have a simple relation to one another, may be considered as the counterpart of Dr PROUT's law; the relation of volumes being substituted for that of weights. Oxygen is the least combining volume, and if its combining volume be called 1, that of all the other simple gases will be 2, of ammonia 4, &c. The numerical expressions for volumes are indeed few, and in that respect differ from the numerical expressions for combining weights; but the principle in both is the same, namely that all the numbers in either series are even multiples of the lowest combining quantity belonging to each.

If the combining volumes of GAY-LUSSAC were to be determined on such rigid experimental principles, as seem to be acted on by the French and Swedish chemists in determining combining weights, we doubt very much whether it could be asserted that oxygen combines with hydrogen in bulk, in the ratio of 1 to 2. Experimental determination can do no more than furnish an approximation to the truth; and in the strict sense, it may be said to be impossible, when analysing water, to obtain the oxygen and hydrogen in volume in the precise proportion of 1 to 2. But the constituents may be obtained very nearly in those proportions; and knowing, as we do, the simplicity of nature's laws, and that gaseous analysis generally gives close approximations to simple ratios, we are satisfied to assume those proportions, which our experimental results so nearly approach. Now this is all very proper; but we would ask, why is not the same course adopted in determining combining weights. We see no difference in the two cases,

except that the principle must be applied with more caution in the case of weights.

We have said more than we first intended on the theory of chemical proportions, and, therefore, shall be under the necessity of being more brief in the remaining remarks which we may have to offer.

Under the compounds of hydrogen and carbon, Dr TURNER gives the composition of olefiant gas to be two atoms of carbon + two atoms of hydrogen. We cannot conceive why the atoms must be duplicated in this case. It is true that we may say that this gas consists, in volume, of two volumes of vapour of carbon + two volumes of hydrogen, condensed into one volume; and that turning these volumes into their equivalent weights, we shall have two atoms of carbon + two atoms of hydrogen. But we would say that its composition in volume is one volume of vapour of carbon + one volume of hydrogen, condensed into half a volume; and these volumes converted into weights, would give us a single atom of each ingredient of the gas. It is true that the condensation of the elements of this gas is anomalous, and it is this circumstance, probably, which induces Dr TURNER to assimilate it to the several curious vapours recently discovered, which are identical with olefiant gas in the proportions of their ingredients, but still more condensed; and the difference between which is supposed to depend by the author, on a greater or fewer number of iterations of the same atoms to form one compound atom. But admitting the probability of this conjecture, as applied to the newly discovered vapours, it seems premature to place olefiant gas in the same class with them, and to assert that the compound of a *single atom* of carbon with a *single atom* of hydrogen is yet to be discovered.

Dr TURNER's book has the merit of being well written, a merit which belongs to few of our chemical treatises. The different subjects are treated of with neatness and brevity, and the author has evinced much judgment in the selection of the facts and statements most appropriate for a compendious treatise. Throughout, much to our satisfaction, the hydrogen unit for proportional numbers is employed, and we do not despair of seeing the day, when this unit will be universally adopted. As a specimen of the author's style, we shall make one extract, which will enable our medical readers to judge on this point for themselves, and at the same time present them with a good abstract of the latest chemical researches on the colouring matter of the blood.

"The crassamentum or clot of the blood consists of two parts, the fibrin and colouring principle. The latter resides in distinct particles, which, according to PREVOST and DUMAS, are elliptical in birds and cold-blooded animals, and assume the globular form in the mammiferous animals. These globules are insoluble in serum; but their colour is dissolved by pure water, acids, alkalies, and alcohol. Much uncertainty prevails among chemists relative to the cause of the colour of the red globules. As soon as the blood was known to contain iron, the peroxide of which has a red tint, the colour of the red globules was ascribed to the presence of that metal, and some chemists supposed it to be in the form of the sub-phosphate of iron. This opinion was adopted by FOURCROY and VAUQUELIN, who even affirmed that the phosphate of iron may be dissolved in serum by means of an alkali, and that the colour of the solution is exactly similar to that of the blood.

"This subject was investigated in 1806 by BERZELIUS, who showed that the

sub-phosphate of iron cannot be dissolved in serum, in the way supposed by FOURCROY and VAUQUELIN, except in very minute quantity; and that this salt, even when rendered soluble by phosphoric acid, communicates a tint quite different from that of the red globules. On comparing together the composition of the three principal ingredients of the blood, viz. fibrin, albumen, and colouring matter, he found that the ashes of the last always yielded oxide of iron in the proportion of 1-200th of the original mass, while the oxide was entirely wanting in the two former. From this it was a probable inference that iron is somehow or other concerned in the production of the red colour; but the experiments of BERZELIUS did not make known the state in which the metal exists in the blood. He could not detect its presence by any of the liquid tests. (Med. Chir. Trans. Vol. III. p. 213.)

"In a series of experiments published in 1812, (Philos. Trans.) Mr BRANDE obtained results quite contrary to those of BERZELIUS. He detected iron in the ashes of the serum and fibrin as well as in those of the red globules; and in each it was present in such minute quantity, that no effect as a colouring agent could be expected from it. Mr BRANDE supposed that the tint of the red globules is produced by a peculiar animal colouring principle, capable, like other substances of a similar nature, of combining with metallic oxides. He succeeded in obtaining a compound of the colouring matter of the blood with the oxide of tin; but its best precipitants are the nitrate of mercury and corrosive sublimate. Woollen cloths impregnated with either of these compounds, and on their being dipped into an aqueous solution of the colouring matter, acquired a permanent red dye, unchangeable by washing with soap.

"The conclusions of BRANDE, relative to the presence of iron in the albumen and fibrin of the blood, received additional support from the researches of VAUQUELIN; (An. de Ch. et de Ph. Vol. I.) but the question has been finally decided by Dr ENGELHART, a young German chemist of great promise, who gained the prize offered in the year 1825 by the Medical Faculty of Gottingen for the best essay on the nature of the colouring matter of the blood. (Edinb. Med. and Surg. Journal for January 1827.) He demonstrated that the fibrin and albumen of the blood, when carefully separated from colouring particles, do not contain a trace of iron; and, on the contrary, he procured iron from the red globules by incineration. But he has likewise succeeded in proving the existence of iron in the colouring matter of the blood by the liquid tests; for, on transmitting a current of chlorine gas through a solution of the red globules, the colour entirely disappeared, white flocks were thrown down, and a transparent solution remained, in which the peroxide of iron was discovered by all the usual re-agents. The results obtained by Dr ENGELHART relative to the quantity of the iron, correspond with those of BERZELIUS. These facts have been since confirmed by M. ROSE, who has accounted in a satisfactory manner for the failure of former chemists in detecting iron in the blood while in a fluid state. He finds that the oxide of iron cannot be precipitated by the alkalies, hydrosulphuret of ammonia, or infusion of galls, if it is dissolved in a solution which contains albumen or other soluble organic principles.

"From the presence of iron in the red globules, and its total absence in the other principles of the blood, it is probable that this metal, though its quantity does not exceed one-half per cent., is essential to the production of the red colour. The experiments of Dr ENGELHART, however, have not determined the

manner in which it acts, nor in what state it exists in the blood, though it is most probably in the form of an oxide. It is a singular coincidence that the sulphocyanic acid, which forms with the peroxide of iron a colour exactly like that of venous blood, has been detected in the saliva. The existence of this acid in the blood itself is, therefore, a circumstance by no means improbable."

The above extract furnishes a fair specimen of the general style of the author; but we are by no means to infer that he possesses the inferior merit only of a good compiler of the labours of others. On the contrary, Dr TURNER, although a young chemist, has already made himself favourably known by several detached experimental memoirs. The estimation in which he is held by the British public as a promising chemist may be inferred from the fact, that he has been lately called from Edinburgh to fill the important professorship of chemistry in the London University.

In perusing Dr TURNER's volume, we have discovered the following errors, which we mention for the benefit of those students who may be reading the work.

Page 22. Speaking of the expansion of water:—"as 4.7 to 15," for, 14.7 to 15.

Page 33. "Thus, if one pound of mercury at 180°" for 185°.

Page 116. "*Silver* retains its lustre," for *gold*, &c.

Page 135. The contraction of volume in the formation of sulphurous acid is stated to be "*to a third*," instead of *to a half*.

Page 169. "That some of the *peroxides*," &c. for, that some of the *protoxides*.

Page 190. "When an acid is added to the *hyponitrate* of potash," for *hyponitrite* of potash.

Page 220. "GAY-LUSSAC found that [sulphuric acid] is resolved into two measures of *sulphuric* acid and one of oxygen," for, *sulphurous* acid and one of oxygen.

Page 225. "According to DULONG, [hydrate of phosphoric acid] is composed of 36 or three atoms of phosphoric acid," for, 84 or three atoms of phosphoric acid.

Page 245. "The protoxide of *hydrogen*" is printed for protoxide of *chlorine*.

Pages 249—250. Speaking of perchloride of carbon, it is said, "alkalies do not act upon it; nor is it changed by the stronger acids, such as the muriatic, nitric, or sulphuric acids, even with the aid of heat; charcoal is separated, and muriatic acid gas is evolved." There is evidently something omitted before the word charcoal, necessary to make the sense complete. The context seems to require these words,—"when transmitted along with hydrogen through a red-hot tube," charcoal, &c.

Page 282,

"Hydrogen . . 4.236 . . 2 or *one atom*,"

for 2 or *two atoms*.

Page 301. "Dr THOMSON states, that water and *sulphurous* acid result," for *phosphorous* acid.

Page 304. "The instant [sulphuretted hydrogen] comes in contact with the cyanuret [of mercury], double decomposition ensues, and hydrocyanic acid and the black sulphuret of mercury are generated." This sentence contains a mistake, if by black sulphuret Dr T. means the proto-sulphuret. The cyanuret of mercury, being a bicyanuret, there is obviously generated a bisulphuret of

mercury, or cinnabar. Further on Dr T. appears to be aware of this mistake; for at page 438, he says, "I apprehend the black precipitate, formed by the action of sulphuretted hydrogen on the cyanuret of mercury is likewise a bisulphuret."

Page 318. "*Bicarburet of sulphur*" is printed for *bisulphuret of carbon.*

Page 353. The process, which is designated as the best for obtaining *lithia*, results in obtaining the *sulphate of lithia.* In a treatise, intended for learners, it ought not to be taken for granted, that they will know how to obtain pure lithia from the sulphate.

Page 358. "108 parts or *one* atom of water" for 108 parts, or *twelve* atoms of water.

Page 362. "The composition of chloride of *calcium*" for chloride of *lime.*

Page 364. Magnesium is said to be "converted by *mixing* with oxygen into magnesia."—Same page, magnesia is stated to be "a white *fusible* powder."

Page 373. "By conducting *fluoric* acid gas," &c. for *fluo-silicic* acid gas.

Page 413. "The peroxide of uranium is decomposed by a strong heat, and converted into the *peroxide*," for *protoxide.*

Page 425. Protoxide of copper is said to combine with sulphuric acid. We never heard of the salt which in this case would be formed, our impression being that all the salts of copper were per-salts, except one, the protomuriate. In the same page, ammonia is said to throw "down a greenish-blue sub-sulphate," without any thing being said in the context about the sulphate of copper.

Page 428, table of the oxides of lead. *Protoxide* is printed for *peroxide*, and 124 for 120.

Page 433. Some of the salts of the peroxide of mercury "are converted by water into insoluble yellow sub-salts and soluble colourless *per-salts*" for *super-salts.*

Page 435. "The chloride of mercury [meaning corrosive sublimate] is probably converted, at the moment of being dissolved, into a muriate of the *protoxide*," for *peroxide.*

Page 485. "The *carbonate* of lime is separated by filtration," for *phosphate* of lime, &c.

Page 490. "The yellow chromate which consists," &c. for the yellow chromate of *lead.*

Page 505. "The hydro-sulphate of baryta and strontia, prepared by dissolving the sulphurets of barium and *strontian* in water," for *strontium.* Same page. "If the alkali is completely saturated with the gas, the resulting compound is a *bisulphate*" for *bi-hydrosulphate.*

Page 521. The water in acetate of soda is stated to be "73 parts or six atoms," instead of 54 parts, &c.

Page 523. The crystals of the binacetate of copper are stated to "consist of *one* atom of acid and *two* atoms of the peroxide of copper," instead of *two* atoms of acid and *one* atom of the peroxide.

Page 527. Oxalate of lime is stated to consist "of 36, or one atom of the acid, and 23 parts or one atom of lime," instead of 28 parts or one atom of lime.

Page 531. "Mr PHILLIPS recommends that 100 parts of metallic antimony in fine powder should be boiled to dryness in an iron vessel, and that the residual subsulphate," &c. After "vessel" the words, *with* 200 *parts of sulphuric acid*, are wanting, to make the sense complete.

Page 564. "In cider, *sherry*, ale and porter," for, In cider, *perry*, ale and porter.

Page 567. "According to this chemist, *sulphuric* acid and the oil of wine are both composed of sulphuric acid and carburet of hydrogen," for, according to this chemist, *sulpho-vinic* acid, &c.

Page 594. "When the gum of the olive *oil*," for, When the gum of the olive *tree*.

Page 601. "The information contained in chemical works relative to the substances susceptible of the *vinous* fermentation," &c. for, *acetous* fermentation.

Page 616. "When fibrin is mixed with its own weight of concentrated sulphuric acid, a perfect solution ensues, without change of colour, or disengagement of *sulphuric* acid," for disengagement of *sulphurous* acid.

Page 687. "*Mode of analyzing an earthy mineral containing silica, alumina, manganese, lime, and magnesia.*" The directions given under this head contemplate the presence of iron also.

Page 690. "Hard water is characterized by decomposing soap, the lime of the former yielding an insoluble compound with the *acid* of the latter," for *oil* of the latter.

Page 704 *et seq.* The table of proportional numbers is carelessly printed. The atoms of oxygen for example, are evidently intended to be denoted by the initial of that element, followed by the atomic weight; thus, o. 8, o. 16, o. 24, &c.; but by a singular mistake of the printer, the whole is invariably printed, as if it were a decimal; thus, as applied to the above instances, 0.8, 0.16, 0.24. Sometimes the *number* of atoms follows the initial; at other times, the *aggregate weight* of the number of atoms intended to be expressed. This want of uniformity in plan is apt to mislead the student.

Besides the above, we have detected a few additional errors of less importance, which we have not room to indicate. Upon the whole, we think it not going too far to assert, that, whatever may be the other merits of Dr TURNER's book, it contains many more errors than can be properly ascribed to the intrinsic difficulties of correcting the press.

Mr John Grigg of this city has lately published an American edition of Dr TURNER's work. We are glad to see this, as we consider the work to be valuable on the whole, and as having the advantage of being neither too copious in the detail, nor too compendious; but we regret very much that the reading public in this country should furnish a demand for books, executed in the form and manner of this reprint, with all its disadvantages of duodecimo size, small type, close lines, and inferior paper. It is perceived that we lay the whole blame upon the public; for we assume it as not to be doubted, that if they desired a better commodity, and were willing to pay the price, it would be furnished with all willingness by the bookseller.

It is to be regretted that the Edinburgh copy had not been revised before reprinting it; as then the errors which we have noticed above might have been avoided. As it is, they have been perpetuated, with one or two exceptions, and indeed, added to by the unavoidable inaccuracies of all new impressions. Though, as we have said before, we cannot approve of the cheap style in which the American edition is got up, yet it is but justice to say, that the type, though small, is good, and the impression, clear and distinct.

We conclude by recommending Dr TURNER's treatise to the attention of our chemical and medical readers; and by those who wish to possess the work, and who may not find it convenient to procure the more costly Edinburgh copy, the American edition will be found a convenient and cheap substitute.

Quarterly Summary

OF

Medical and Surgical Intelligence.

I. ANATOMY.

1. *Dr* WEBER *on the Skin.*—Dr WEBER is professor of anatomy at Leipzig. In opposition to the opinion of Dr EICHORN whose observations were cited in a former number of this journal, Dr W. asserts that the sebaceous follicles of the skin are organs distinct from the *bulbs of the hair,* and that they exist over the whole surface, excepting the palms of the hands and soles of the feet. The bulbs of the large hair (gros poils) are situated very deeply in the derm, and sometimes penetrate even into the subcutaneous adipose tissue: the sebaceous follicles on the contrary, are nearer to the cutaneous surface, and are never found extending to the adipose structure. Their size also, says he, is too large, to permit them to be confounded with the bulbs of hair, which are much smaller. In new born children, sebaceous follicles may be discovered on all parts of the skin with the two exceptions already named. The skin of the scrotum shews them very much developed; each of these follicles is composed of four or five compartments or cells agglomerated together; their transverse diameter exceeds their depth. The greatest diameter observed by the author was a quarter of a line.

Dr WEBER also examined the hair of the body in order to ascertain whether it is hollow, but he found no canal, or any cellular structure within it. Neither is this hair cylindrical; it is rather elliptical or oval like the negro hair, in which the flattened form seems necessary to admit of the curl or twist.

2. *Singular Case of Mutism.*—A young man, about twenty years of age, fell two years since from a height of ten or twelve feet; he lighted on his heels, and by the *contre-coup* experienced a concussion of the brain, was stunned for half an hour, and ceased to speak freely. After two years he was brought to the Hotel-Dieu, as affected with hemiplegia. M. DUPUYTREN received him, and soon ascertained that he was affected neither with hemiplegia nor paraplegia. The young man could walk and use his arms, but on attempting to answer a question he hesitated, manifestly from consciousness of the difficulties he had to encounter. He made vain efforts and could scarcely get out a few words badly articulated. There was nevertheless no appearance of idiotism. He appeared to understand the words addressed to him.

Is the disease local? asks M. D. Does it depend on some particular affection, (*vice propre*) on a paralysis of the tongue? or does it arise from an affection of the brain at the origin of the nerves that move this organ? Is there paralysis of both sense and motion, or does he still retain the power of distinguishing savours? The solution of this last question would not be difficult, and M. D. supposed that the solution of this might involve that of the former.

If the patient still preserved the perception of savours, if the nerves of motion only are injured, the cause of the affection might be found in the part of the brain that gives origin to them: if, on the contrary, sensation is gone, if taste has disappeared, then, the nerves of taste being different from those of motion in their origin, it will be difficult to conceive of a simultaneous affection of two parts so distant from each other. In that case we must seek for the cause and seat of the disease in the nerves of the tongue, in the organ itself.

M. DUPUYTREN now caused the young man to be taken to the amphitheatre: he was pale, lean, of a common height; his countenance a little embarrassed; he appeared to be at once bashful and pensive. He was asked the nature of his profession. *Mine - - -* replied he with difficulty; at length he was enabled to get out *Mineur*. His age? *Vingt - - - t - - - t - - d - - eux ans*. His name? *Jacques Col - in - - Col - - ar - - Col - - ard*. Have you any brothers and sisters? *Oui*, said he. How many brothers? *Un*. How many sisters? *Trois*. Are they married? *Oui*. What is the business of your brother? *Peintre*. This word was pretty well pronounced. What is the business of your eldest sister's husband? *V - - V - - Ver - - -*, said he. Is it *Vitrier* you wish to say? asked M. DUPUYTREN. He makes a negative gesture. Does he make bottles? Same sign—*V - V - Ver*—repeats the patient. Do you understand well what I say to you? *Oui*. Strike on the table. He struck. Lift up your foot. He lifted it up. Put it to the ground. Turn your head to the right—to the left. Look up at the sky—down to the ground. Get up. Go to the right—to the left—straight forwards. Come back. Sit down. Put on your cap. All these orders were obeyed immediately, and all the motions executed with promptness and precision.

"The *difficulty* seems to be in the tongue only," said M. D. "*Non*," replied the patient, thrusting out the tongue to shew it was free and sound. "The *difficulty* is in the tongue, the muscles of the larynx are doubtless not connected with it; it is the organ of speech, the modificator of sounds, that is affected: the brain appears to be free also."

"Let us continue the examination; pronounce the words that I am going to say to you—*Sa, se, si, so, su*." He pronounced them without much difficulty, except the *so* and *su*, which his tongue refused to repeat: he could only say *o—u*. "Say *mon pêre*." He answers "*pé - - -*." "*Ma mêre*." He answers "*mé*." "Do you retain the sense of taste?" said M. DUPUYTREN. "*Oui*." "Do you understand my question? When you get soup does it taste as it used to? Does some food taste good, and some bad? Can you taste salt and pepper?" "*Oui*." "We will ascertain that point. We will make the trial at the next examination" (seance): and the young man, with more haste than politeness, began to leave the place, without saying any thing. "Stop, my friend, is this the way you intend to leave these gentlemen? Pull off your cap, and bid them adieu." He takes off his cap. "Well, say adieu." "*Ad - - - d - - - eu*," said he, and left the room.

April 9. The trials intended for Monday were deferred till this day (Wednesday). The young man appeared to dread them, and submitted to them reluctantly. Three substances were to be employed—salt, sugar, and pepper. In the first place, M. D. wished to ascertain whether he could pronounce these words sufficiently well to obviate the danger of attributing to a defect of taste what might depend only on a fault of speech. He said *sel* very well; the words *sucre* and *poivre* were pronounced imperfectly. Some salt was then placed on his tongue, and he recognised this substance: but he afterwards mistook sugar for salt, and called the pepper *rum*. Some one remarked that he was from a district where little wine, but a great deal of brandy was drunk, which made M. D. think he had confounded the taste of rum with the biting quality of pepper. He appears nevertheless to know the taste of wine, which he has asked for several times since he has been in the Hotel-Dieu. He was next made to drink some water, which he swallowed and recognized without any difficulty. Recollecting the observations frequently made after concussion of the brain, chronic affections of the organ, and apoplexies, M. D. is tempted to modify, or rather, completely to

change the opinion he had formed in the onset. After affections of the brain we in fact frequently observe both loss of memory and forgetfulness of things and words, of proper names, of substantives or adjectives; some patients lose the faculty of comparing objects and forming a judgment of them. We have seen an old woman who after an attack of apoplexy, followed it is true by hemiplegia, although she retained the free use of her intellectual faculties, and understood all the questions that were addressed to her, yet could only answer by these words frequently repeated—*Saint Antoine, Saint Antoine.* After all, in these different patients the intellectual faculties appear to be, in general, sound; there appears to be no other lesion of the senses, and we might according to M. DUPUYTREN attribute the affection to a general lesion of the brain, influencing secondarily the functions of this or that sense, rather than to a direct and local affection of two different points of one organ.—*Nouvelle Bib. Med. Avril.*

II. PHYSIOLOGY.

3. *Rumination.*—M. RICHE of Maubeuge, writes to M. BROUSSAIS, that a young man aged seventeen years and a half, in pretty good health, is affected in the following manner. From the age of nine years he has observed that his food comes back to the mouth after being swallowed, without having undergone scarcely any change; this generally takes place in about half an hour. He swallows it again without a sensation of either pleasure or disgust. He can, if smoking, or by an exertion of will, prevent this singular ascent from taking place, but then he soon finds that pain occurs in the epigastric region. He is always hungry, eats with pleasure both animal and vegetable food, but has a slight preference for the former. His drinks also come back to the mouth but give him no pain.—*Annales de la Med. Phys. Avril.*

4. *State of the Eyes in Amaurosis.*—The eyes of a person affected with amaurosis were examined by Professor HEUSINGER, and both optic nerves were in a state of atrophy, though the eyes were well formed and of the natural consistence. The sclerotic contained a considerable quantity of yellow coloured humour. The choroid and retina natural. The vitreous humour had disappeared, the lens was yellow and opaque. Three translucid vesicles as large as a pea and of a pyriform shape adhered to the outer surface of the retina. They contained a fluid similar to that which escaped in puncturing the sclerotic, and which Professor H. thinks was contained in similar vesicles. He thinks they were formed by Jacobs' membrane.—*Lancet, May 10th.*

5. *Spontaneous Combustion.*—Professor RUDOLPHI relates a case in the Journal de Pharmacie, of a man who suddenly felt a pain in the arm as if he had received a blow from a stick, and immediately perceived at the same spot a small flame which burnt his shirt. We extracted for a former number of this Journal the case of a young girl whose finger took fire. It is said that the best electrometers gave no evidence of electricity when placed near her finger.—*Lancet, May 17th.*

6. *Case of Amnesia.*—Dr CHAILLY in the Journal Générale, relates the case of M. Simon about 57 years of age, who was playing at the game of *tric-trac* in a warm room, felt a sudden pain in the left temple, and at the same moment lost the power of expressing his thoughts; his memory betraying him every time he attempted to use a substantive word, all words of that kind being replaced by *sonnez & six-cinq*, terms used in the game of tric-trac. He was taken home, and Dr CHAILLY being called, found him up, his face flushed, and complaining by gestures, of a pain in the left temple, but unable to say further than *J'ai là un sonnez.* Every thing was *sonnez* or *six-cinq*, with him. He

called his daughter by one of these names. He was bled both generally and topically, and used derivatives, by which he recovered.—*Nouvelle Bibl. Med. Avril.*

7. *On the Functions of different parts of the Ear.*—Dr Charles L. Esser, in a work crowned in 1825 by the Medical Faculty of the University of Bonn, has been led by numerous experiments to the following conclusions.

The *cartilage of the external ear* seems to contribute little to rendering sounds more clear, but seems to augment their force not only by reflecting into the auditive canal a part of the sonorous rays, especially those that fall into the concha, but also by means of vibration which the sonorous rays produce in it, and which it transmits to the tympanum.

The *bones of the head* contribute no less than the cartilage to the propagation of sounds. This propagation does not depend solely on the nerves as was thought by Treviranus and Swan, &c. for a watch applied to a swelled cheek ought to produce sounds as clear through the medium of the facial nerve, as if applied on the zygomatic arch, which is not the case. The os occipitis is better fitted for propagation of sounds than the bones of the anterior parts of the head which depends on its connection with the labyrinth, and its nearness to the mastoidal cells. The use of those cells is not to prevent an echo in the internal ear as M. Treviranus supposed; that function devolves solely on the eustachian tube. In many animals the bones that surround the external ear are disposed so as to favour in a high degree the transmission of sounds, and they offer compensation for the absence of an external ear.

The external auditory canal is evidently the part which contributes most to the concentration and transmission of sounds to the membrana tympani.

The membrana tympani is made to vibrate by the sonorous rays that reach it; nevertheless this is not the only use of the membrane, for the sonorous vibrations not only may reach the ear without the aid of the membrana tympani, but may even reach it with greater strength: it serves to protect the ear from external injuries.

The eustachian tube is the chief auxiliary to the membrana tympani, and fulfils four different functions. 1. It allows the air contained in the cavity of the tympanum to be in a state of equilibrium with the external air. If this equilibrium be lost, certain anomalous sensations are experienced, such as tinkling and buzzing of the ears. If the quantity of air in the cavity is augmented by strong expirations, then too strong a pressure on the membrane of the tympanum takes place as well as on the other parts of the cavity, particularly the *foramen rotundum:* this pressure produces *buzzing*, which decreases in proportion as the atmospheric equilibrium is restored through the eustachian tube. If the air of the cavity becomes rarefied, and the eustachian tube closed by spasm, then the external air pressing on the membrane finds its way through the pores of that organ, and thus produces the tinkling of the ears.

Both these phenomena disappear as soon as the equilibrium is restored in the cavity, and this may be effected by forcing the air along the eustachian tube by breathing with the nose and mouth closed, or by introducing the end of the little finger deeply into the external meatus and withdrawing it by degrees, making at the same time pressure against the upper surface of the canal. In this way a vacuum is formed, the membrane is brought back towards the external meatus and the eustachian tube gives passage to the air that presses into it from the fauces. This explanation manifestly can only apply to the occasional occurrences of buzzing, &c. Chronic examples of these affections depend on cerebral congestions or anomalous action of the nerves.

2. The second function of the eustachian tube is to admit of the occurrence of vibrations in the cavity of the tympanum, which could not be the case if it were entirely closed.

In cases of deafness occasioned by obliteration of the eustachian tube, perforation of the membrane becomes a means of cure by re-establishing the communication with the external air.

The idea that the sides of the tube are constantly in contact with each other is incorrect.

3. The tube prevents confused vibrations of the air of the cavity by giving them a passage outwards.

4. Finally, it serves to lead into the posterior nares the mucus secreted into the *cavity* and on its own surface.

The small bones of the ear, by means of their muscles, are capable of rendering the tympanum more or less tense; but, it is difficult to conceive why and how this effect takes place. Their influence on the sense of hearing is not very remarkable. They serve to transmit the sonorous vibrations of the membrana tympani to the foramen ovale, although this is not their sole use.

The labyrinth, whose anatomical history is sufficiently understood, is still, and will probably continue to be the obscurest part of the apparatus of hearing, as relates to its physiology. The experiments of WEBER leave it a doubtful question whether the lymph of COTUGNI exists during life, or is a production occasioned by the death of the animal.

The vestibule or the membranous bags which represent it in certain animals, and the semicircular canals appear to contribute most to the sense of hearing; but it is difficult to say in what manner. Some facts in comparative anatomy seem to shew that the principal use of the semicircular canals consists in strengthening the sounds. They are larger in animals whose external ear is less favourably formed or wholly wanting—large semicircular canals are generally accompanied with a small cochlea and *vice versa*.

The cochlea appears to be of less importance than the semicircular canals, for it soon disappears in the descending scale of animals, and birds only possess a rudiment of it. Its use appears to be, to offer a larger surface to the sonorous vibrations, and to strengthen by concentrating them.

The act of distinguishing different objects is a purely intellectual function which we must not seek for in any particular parts of an organ of sense. This power has been supposed to reside in the cochlea; but if its devolopment was in every case to be regarded as a measure of this distinguishing power, then the following order might be established. The cabiais and porcupine whose cochlea has three turns and a half; the dog and fox with three turns; man, the cow, the hog, and cat with two and a half turns; the horse and the dolphin with two and a quarter; and the hare with two turns: birds would occupy the lowest rank in this series. Such facts need no comments.

The part which the *acoustic nerve* plays in the sense of hearing is undoubtedly of great importance, but its manner of acting will always remain an impenetrable mystery.

The author of this interesting memoir finally arives at the conclusion that all parts of the auditory apparatus concur in the act of sensation; but the sense of hearing itself is not explicable by means of these parts, for like all the other senses this is purely intellectual. The soul alone sees and hears; the rest is both blind and deaf. *Bulletin, April.*

8. *Five Children at a Birth.*—A Russian peasant woman who had before had twins was delivered of five children in November 1824; to wit, she gave birth to a daughter on the 9th, 10th, 12th, and 13th respectively, and to a son on the 16th of the month. The children were each about eight inches in length. The son was born dead; the daughters died on the 6th day: as for the mother, she was restored to health at the end of a month.

III. PATHOLOGY.

9. *Dr* J. A. SMITH *on Medical Theories.*—Dr SMITH has published, in the New York Medical and Physical Journal, an eloquent "lecture on Medical Philosophy;" on which we are led to offer some remarks, from the great influence which the acknowledged weight of his character must possess with a large portion of the American profession. Dr S. appears to have been already misunderstood, as he states in his introductory note; and indeed we think, under these circumstances, a small explanatory addition, by way of guarding his readers from the same errors into which his hearers had fallen, would not have been amiss. With the general course of his opinions we can fully concur. All that he says of the folly of hypothesis, and the necessity of a close adherence to fact and observation, is most true and of the highest importance. All that we have to object to, therefore, is what we conceive to be an erroneous application of these principles.

Dr S. appears to start with the *hypothesis* that the present age is similar to the last; and that, because former medical theories have been conjectural and perishable, it follows that the same will be the case with the doctrines which have supplanted or are supplanting them, at the present day. Thus, at page 176, the words "membranes, surfaces, tissues, and contrastimulants," are termed "the cant phrases of the day, and the legitimate successors of excitement, excitability, state of the system, &c." and to discourse with fluency of them is pronounced to be "very easy, very captivating, very imposing, and very useless;" and these considerations are viewed as differing widely from the collection of dry details, the methodical arrangement of these, and the ultimate deduction of inferences by patient and continued thought. The subject of contrastimulism we, for the present, will waive; but with regard to membranes, tissues, surfaces, &c., we are really ignorant where the facility of hypothesis described in these sentences is to be met with: certainly not in the vast collections of anatomical and physiological facts made by BICHAT, nor in those elaborate series of cases and inferences presented to us in the practical writings of LAENNEC, BROUSSAIS, LALLEMAND, and ABERCROMBIE! To convict the solid medicine of modern times of being conjectural and hypothetical, would, we apprehend, be a difficult task, and require a more extended analysis of it than that presented by Dr S. The phrases, too, which he has designated as cant, possess at least this advantage over their predecessors, that they express something *tangible*, something with regard to which there is much more facility in demonstrating and establishing facts, than in the former. One can grasp a "membrane," a "tissue," or a "surface," in his fingers, or in a forceps, and hold it up to the light of the sun; while it would not be easy to do the same by "excitability," or "the state of the system." The colour or thickness of a "tissue" is a *fact*, capable of being made apparent to all who possess the sense of sight in sufficient perfection, with sufficient knowledge of the subject to compare what they see; while the state of "excitability" is a matter of opinion about which men of the highest attainments may differ.

Yet, notwithstanding this preference, we are of opinion that the doctrines to which Dr S. refers, as gone by, are not without their value in true medical philosophy. We think they cannot with justice be considered as overturned: they are only confined within their proper limits. Are we to suppose that the reflections of a man of genius and an observer are devoid of all utility, because he extends them to cases which, in consequence of human fallibility, he does not understand? The very doctrines here alluded to are things which have their foundation in nature; and we have only to guard against an improper extension of them. The age is in reality progressive; the experience and reflections of one man are capable of being added to those of another, and human science is, in medicine, as in every other branch, the product of the labour of many minds.

The real truth we apprehend to be this. The presumption that the present age and its opinions are like those of former times, is *erroneous*. That portion of time which has elapsed since the American Revolution is distinguished from all others by a rapidity in the extension of demonstrable science unexampled in

the history of civilized man. Within that period sciences which were previously in existence have been tripled in extent, while numerous new ones have been created, of which our forefathers never dreamed. Zoology, botany, mathematical astronomy, chemistry, geography, the science of government, have undergone extensions and accessions far beyond the value of their whole previous amount. Electricity, galvanism, electro-magnetism, meteorology, mineralogy, geology, political economy, the knowledge of the polarization of light, the discovery of the Egyptian hieroglyphics, all these and more, so numerous that memory refuses to furnish their names, without a longer warning, are among the glories of the present age. This is the epocha of constitutions and steam-boats, of London Universities, of United States and public opinion. There never existed such a period; and that which most nearly approached to it, the time when Greece was free and independent, was also distinguished for mental improvement and discovery. "We live," says RUSH, "in a revolutionary age; and our science has caught the spirit of the times." In our own department of knowledge, great advances have surely been made in physiology and surgery; and the admission of this ought to suffice, were the fact not directly demonstrable, to rebut the a priori supposition that no solid advance had been made in matter of such deep concern as medical doctrine.

We conceive that the improved extent and solidity of medical doctrines at the present day, is owing to the same leading cause which has produced the truly surprising results in public affairs which we have above enumerated. This is the *increased number of cooperators*. Revolutions in governments were got up by inducing the whole mass or the great majority of a nation to attend to its affairs; and circumstances of a similar character operate in medicine. Never before were there so many educated physicians employed in endeavouring to improve their science; nor did there ever exist such numerous and rapid means of communication. The immense multiplication of medical books, and, in particular, of medical journals, disseminates the knowledge of new facts to a larger number of individuals than at any former time; and there are, in consequence, more numerous competitors for discovery, and more frequent critics on each others' errors and extravagancies. That which makes a strong impression on public medical opinion at the present day, may be safely depended on for containing a far larger proportion of fact, and a smaller share of absurdity than common in former ages. The authors of new opinions know that they will be more freely canvassed, and that themselves need never aspire to become the objects of that idolatry which was so freely bestowed upon the great names among their predecessors. And no small portion of the credit of thus augmenting useful human sciences will be found to belong to the much debated name of Dr BROUSSAIS. We freely acquit Dr SMITH of any willingness to furnish ignorance and indolence with a convenient excuse for not studying new opinions, which it may be too laborious to investigate; but let him consider the influence which his ideas will have upon the minds of others; and he will perhaps fear to add to those difficulties which a foreign language, prejudices of education, and a laborious occupation throw in the way of the diffusion of modern improvements among our countrymen.

While the above journal is still in our hands, we observe that Dr T. ROMEYN BECK has stated, at page 201, that he "had not been able to obtain any authentic account" of the Pennsylvania Hospital, as an asylum for the insane. We are surprized at this; and would refer the now celebrated Medical jurist to the Philadelphia Literary Gazette, 1821, No. 15; to CHAPMAN's Philadelphia Journal, Vol. IX. page 30; to a Description of the Hospital, published in the present year, by Mr WILLIAM G. MALIN, clerk to the institution; for one of a peculiar documentary character, to that rich and valuable collection, HAZARD's Register of Pennsylvania, Vol. II. Nos. 6 and 7; and to the printed annual accounts of the establishment.

10. Dr ABERCROMBIE *on Apoplexy*.—In despair of being able to find space for a review of Dr ABERCROMBIE's invaluable *Researches on Diseases of the Brain*, and in consideration of our having already inserted one on the same subject, from the writings of LALLEMAND and BOUILLAUD (Vol. IV. page 106.),

we have thought it best to notice the one first mentioned in this summary. A part, at least, of that matter which we might have compiled from Dr A. is rendered superfluous by the article to which we have above alluded. Dr ABERCROMBIE's work is well calculated to support, in this respect, the reputation of the English school; abounding in cases, (too many, it seems, for the taste of some of the reviewers,) in well ascertained facts, and in careful deductions; and displaying an acquaintance with the labours of the continental physicians certainly beyond that of some of his countrymen, and which, if Mr CHARLES BELL retain the sentiments he once expressed, must, we should think, be truly distressing to him.

Dr ABERCROMBIE divides apoplexies into three species or forms: first, those which are immediately and primarily apoplectic; secondly, those which begin with a sudden attack of head-ach, and pass gradually into apoplexy; and thirdly, those which are distinguished by palsy and loss of speech, without coma.

"In the first form of the attack, the patient falls down suddenly, deprived of sense and motion, and lies like a person in a deep sleep; his face generally flushed, his breathing stertorous, his pulse full and unfrequent, sometimes below the natural standard. In some cases convulsion occurs, in others rigid contraction of the muscles of the extremities; and sometimes contraction of the muscles of one side, with relaxation of the other. In this state of profound stupor, the patient may die after various intervals, from a few minutes to several days; or he may recover perfectly, without any bad consequence of the shock remaining; or he may recover from the coma, with paralysis of one side. This paralysis may disappear in a few days, or it may subside gradually, or it may be permanent. Other functions, as the speech, may be affected in the same manner, being speedily or gradually recovered, or permanently lost; and recovery from the apoplectic attack is sometimes accompanied by loss of sight.

In these cases, where they prove fatal, Dr ABERCROMBIE has found either extravasation of blood or effusion of serum, or as in some instances, no morbid appearance whatever. The latter species he chooses to designate as *simple apoplexy;* and, after various discussions, comes to the following conclusions:

"1. There is a modification of apoplexy which is fatal, without leaving any morbid appearance that can be discovered in the brain.

"2. There is another modification, in which we find serous effusion, often in small quantities.

"3. The cases which are referrible to these two classes are not distinguishable from each other by any such diversity of symptoms as can be supposed to indicate any essential difference in their nature.

"4. Without any apoplectic symptoms, we find serous effusion in the brain in an equal or in a greater quantity than in the cases of the second modification.

"5. It is therefore probable that, in these cases, the effusion was not the cause of the apoplectic symptoms.

"6. It is probable that the cases of the first modification depend upon a cause which is entirely referrible to a derangement in the circulation in the brain distinct from inflammation.

"7. It is probable that the cases of the second modification are, at their commencement, of the same nature with those of the first; and that the serous effusion is to be considered as the result of that peculiar derangement of the circulation which constitutes the state of simple apoplexy. In other words, it is probable that the affection which has been called serous apoplexy is to be considered as simple apoplexy terminating by effusions."

The second form of the disease begins with a sudden attack of pain in the head; the patient becomes pale, sick, and faint, generally vomits, and frequently, though not always, falls down in a state resembling syncope; the face pale, the body cold, and the pulse very feeble. This is sometimes accompanied by slight convulsion. In other cases, he does not fall down; the sudden attack of pain being only accompanied by a slight and transient loss of recollection. The patient continues for some time, perhaps an hour or two, cold and feeble, with cadaverous paleness of the countenance; his pulse weak, and generally frequent. He is quite sensible; but oppressed. By degrees, he recovers heat and the na-

tural appearance of the countenance; and the pulse improves in strength. The face then becomes flushed; he is more oppressed; he answers questions slowly and heavily; and at last sinks into coma, from which he never recovers.—Death took place, in three cases of this form, at the respective periods of twelve hours, forty-four hours, and five days.

From the above description, fatality would seem to be a necessary character of the present variety of disease. Dr A., however, does not mean it so—these cases are *generally* fatal. He considers them as depending upon the immediate rupture of a vessel, without any previous derangement of the circulation; the rupture arising from disease of the artery at the part which gives way. The author compares them to those cases of concussion of the brain, in which the patient recovers from the first shock of the injury, but sinks under a subsequent bleeding, to be either relieved by the trephine, or to pay the forfeit of his life. Dr A. supposes that the brain undergoes a temporary disturbance of its functions, as the immediate effect of the arterial or venous rupture; and that this is recovered from; when, finally, either the bleeding progressively accumulates to an amount capable of suspending the actions of life by its *pressure*, or the vascular rupture is at first closed by a coagulum, for a time, but bursts out afresh and is rapidly fatal. The effusion of blood has, in some cases, been traced to arterial trunks; in others, to the rupture of the sinuses, of small aneurisms, &c.; while in another set, our author subscribes to the French opinion familiar to our readers, that the blood is frequently poured out by *exhalation*, without any rupture. He has found ossifications disposed in a singular series of long rings, separated by small intervening portions of the common appearance. This occurred particularly in elderly people.

At the ossified part, there is frequently a contraction of the cavity of the vessel; and sometimes a total obliteration. In other cases, the inner coat of the artery has been much thickened and softened; and capable of being pressed out from the divided extremity by the fingers.

In the third species, a singular example of the imperfections of our existing nosologies, an *apoplexy* distinguished by *palsy without coma*, Dr A. includes cases which resemble each other in their symptoms, but differ widely in the morbid phenomena observed in dissections.

"The attack appears under various forms; the most common of which is hemiplegia with loss of speech. But in some cases the speech is not affected; while in others the loss of speech is at first the only symptom. In some cases, again, one limb only is affected; which is commonly the arm.—Numerous other modifications occur; as palsy of one eyelid, or of the orbicularis of the eye, double vision, loss of the power of swallowing, &c. The patient frequently appears quite sensible of his situation, and makes anxious attempts to express himself; generally understanding, in a great measure, what is said to him, and answering by signs. Sometimes, however, when he retains a partial power of speech, his answers are incoherent and unintelligible. In the farther progress of cases of this class, we observe remarkable varieties, which may be chiefly referred to the following heads:—

"1. Such an attack may be merely the prelude to the apoplectic, and may pass into it, after a short interval. These cases belong chiefly to the second class.

"2. The attack may, under the proper treatment, pass off speedily and entirely, leaving, after a very short time, no trace of its existence.

"3. The recovery may be very gradual, the use of the affected limbs being restored after several weeks or months.

"4. The palsy may be permanent; that is, the patient, after a certain time, may recover so far as to be able to walk about, dragging his leg with a painful effort, and to speak very imperfectly, and, after this, makes no further improvement to the end of his life; which may be protracted for many years.

"5. In a fifth variety, the patient has no recovery; he is confined to bed, speechless and paralytic, but possessed of his other faculties in a considerable degree; and dies gradually exhausted, after several weeks or months, in some cases without coma, in others with coma a few days before death.

"In endeavouring to investigate the morbid conditions of the brain which are connected with these varieties, we find considerable diversities, which, in a pathological point of view, may be referred to the following heads:

"1. Many of the cases appear to have a close analogy to simple apoplexy; and, when they are fatal, present either no satisfactory appearance, or only serous effusion, often in small quantity.

"2. Extravasation of blood of small extent, contained in defined cysts in the substance of the brain or under the membranes.

"3. Ramollissement of the cerebral substance.

"4. Inflammation and its consequences."

Our author's account of the absorption of blood from the apoplectic deposit does not materially differ from that given by the best modern pathologists. Indeed he tells us that he has not found the cysts to be ultimately obliterated, as "the French writers believe." This is admitted, it is true, by Dr SERRES; but we had understood it to be the prevalent French opinion that apoplectic caverus did not become obliterated. Certainly such is the expressed belief of Drs MARTINET and ROCHOUX, the only writers of this class whom we have at hand. There may, however, be foundation for it, as, to do the French anatomists justice, they have generally appeared to us to be characterised by superior knowledge of the subjects of which they treat, rather than by their entertaining any particular set of opinions. So far from "the French" being a body of men who maintain an exclusive code of opinions, we have thought that there existed among them quite as much discussion and difference of sentiment as in other nations.

Dr A. describes softening of the brain (why does he use the foreign term "*ramollissement?*") in nearly the same manner as his fellow-labourers of the continent. Vide the Review already referred to. He compares it to gangrene, and refers it to *inflammatory action*, or to the *failure of the circulation* depending upon disease of the arteries. His cases differed from M. ROSTAN'S; not being confined to patients of an advanced age. In cases of palsy, Dr ABERCROMBIE has met with the eight following morbid alterations:—

"1. Simple and recent inflammation of the cerebral substance.

"2. This inflammation passing into softening.

"3. The encysted abscess of the brain.

"4. Induration of a portion of the cerebral substance.

"5. Extravasated blood in the ventricles, on the surface of the brain, and in cavities or cysts in the substance of the organ.

"6. The empty cysts from which extravasated blood has been absorbed.

"7. Serous effusion on the surface of the brain.

"8. Extensive disease of the arteries of the brain."

Dr A's *treatment* is exceedingly simple; and we are affected with both pleasant and unpleasant emotions at finding that his experience, enlightened as it is by a clear and certain pathology, has led him to the same conclusions which have been forced upon a large number of practitioners by the facts of daily observation alone. We are glad to find clinical induction confirmed, while we are sorry that there is no very important accession made to the curative means. Still, however, we should not feel disappointed in our hopes of gaining useful practical results from this more close study of the disease. By means of an improved diagnosis, we may succeed in applying and apportioning well known and simple remedies with more discrimination and consequently more success; a power, which, rather than an extended acquaintance with drugs, or even a large and indigested mass of routine experience, constitutes the difference between the skilful and the commonplace practitioner.

"We have also seen grounds for believing that the cases which terminate by effusion are probably, at their commencement, in this state of simple apoplexy. We have seen, farther, that we have no certain mark by which we can ascertain the presence of effusion; and, finally, we have found, that even extensive extravasation of blood in the brain may be entirely recovered from by the absorption of

the coagulum. These considerations give the strongest encouragement to treat the disease in the most active and persevering manner. They teach us, also, not to be influenced in our practice by the hypothetical distinction of apoplexy into sanguineous and serous; and finally, not to be hasty in concluding, in any particular case, that the disease has passed into a state in which it is no longer the object of active treatment.

"In the treatment of apoplexy our remedies are few and simple. Those in which our chief reliance is to be placed, are, large and repeated blood-letting, active purgatives, and cold applications to the head, aided by an elevated position of the body, cool air, and the absence of all stimuli. Antimonials may occasionally be useful as an auxiliary, from their known effect in restraining vascular action; provided, in the early stages, they do not occasion vomiting.

"Our first great object is to take off the impulse of blood from the arteries of the head, by bleeding carried to such an extent as shall powerfully and decidedly affect the system, and by repeating it, at short intervals, as soon as these effects begin to subside. The first bleeding should probably be from the arm; but, after this, there seems to be an evident advantage in abstracting blood locally either from the temporal artery or by cupping. Much importance has been attached by some to bleeding from the jugular vein, as most likely to give immediate relief to the head; but we must remember that the only jugular vein which can be opened, is the *external* jugular, which has very little communication with the brain; and consequently, that bleeding from it is probably much inferior to bleeding from the temporal artery."

We would here ask, in what respect is the communication between the temporal artery and the vessels of the brain closer than in the case of the external jugular vein?

"As soon as possible after the bleeding, means are to be taken for inducing strong purging. The most efficient remedy, by far, for this purpose, is the croton oil; and, if the patient cannot swallow, it may be very conveniently introduced into the stomach, suspended in thick gruel or mucilage, through an elastic gum tube. The operation should be expedited by strong purgative injections. This is always to be considered as a most important and leading point in the treatment of apoplexy; and though, in arresting the progress of the disease, our first reliance is upon large and repeated bleeding, the first decided improvement of the patient is generally under the influence of powerful purging. The effectual application of cold to the head is the third remedy on which we rely, and it is equally applicable to the different states of the disease, whether arising from simple apoplexy, or from extravasation. It may be applied either by means of cold water, or pounded ice in a bladder, or by a full stream of water directed against the crown of the head, and received in a basin held under the chin, while the patient is supported in a sitting posture. I have formerly given an example of a patient restored in a few minutes or rather seconds by this remedy from a state of perfect apoplexy.

"The active use of these remedies is sometimes followed by a very speedy removal of the apoplectic state. In other cases, though little immediate effect may be produced, yet, by a persevering repetition of them, the coma begins to subside after some time; perhaps a good many hours, or even several days. In other cases, again, they may be used in the most active manner without relieving the patient; and, after all, we may find, upon dissection, that the disease was still in the state of simple apoplexy."

Dr A. proceeds to recommend vigour and perseverance; and mentions a case in which upwards of a hundred ounces of blood were abstracted, upwards of twenty drops of croton oil given, and the case terminated favourably, after a very doubtful state of three or four days' duration. He thinks that there is no class of symptoms which characterize a species of apoplexy, in which the treatment should be essentially changed; none, for example, which hinge upon the supposition of a *serous apoplexy* caused by debility. On reference to his cases,

he finds weakness of the pulse and paleness of the countenance to be frequent symptoms of the worst form of sanguineous apoplexy; and, on the other hand, that cases which were accompanied by strong pulse and flushing of the countenance have terminated by serous apoplexy. In one remarkable case, there existed every circumstance that could lead him to consider the disease as serous apoplexy, but the patient died, without any effusion; while, in another, there was most extensive effusion without any apoplectic symptom. He here adds the remark that "in apoplexy the strength of the pulse is a very uncertain guide; for that nothing is more common than to find it upon the first attack of apoplexy, weak, languid, and compressible, and becoming strong and full after the brain has become in some degree relieved by large blood-letting."

11. *Instrumental Percussion of the Thorax.*—The invention of a mode of making percussion produce a louder sound, by means of a small piece of wood, a contrivance of Dr P. A. PIORRY, an Adjunct of the Faculty of Medicine at Paris, is probably known to many of our readers. Dr P. has now produced a volume upon the various applications of which this instrument is capable; and has furnished a surprising number and variety of these. They form a natural and valuable appendage to the copious means of analysis afforded us by the happy researches of M. LAENNEC; of which they are, in numerous instances, only an extension. Not satisfied, however, with the investigation of the diseases of the thorax, M. PIORRY has directed his attention to the employment of the same instrument over the remaining portions of the body; as Drs KERGARADEC, LISFRANC, and others have done by the stethoscope.

LAENNEC had called the use of his invaluable instrument by a phrase which has been translated "*mediate* auscultation;" and the expression of Dr PIORRY is called "percussion médiate." Reference to a dictionary will show that the above English expression is incorrect; as *mediate* does not signify acting *by* a means, but acting *as* a means.

The instrument of our author is called by him a "*plessimetre*"; from πλήσσω, I strike, and μέτρον. A Hellenistic friend furnishes us the remark, that it should have been taken from the aorist ἔπληξα, and not from the present; which would give us, in English, *pleximeter*.

We present these verbal criticisms, in themselves unimportant, from an aversion to a species of servile copying too prevalent among our journalists and other writers, which leads them to adopt the modes of expression used among our European contemporaries, without inquiring whether they are either correct in the original, or consonant to the genius of our own language. It is thus we see "M. such-a-one," used instead of Mr, or what is more proper Dr. This practice is an acknowledgment of inferiority. A manly avowal of the real fact, when it is actually the case, is both honourable and philosophical; but we see no reason for degrading the language in a manner which seems to imply that it is unfit for scientific use. The English is capable of any application; and foreign words, where necessary, should be translated or anglicised, following the Anglo-Saxon spelling, or, where this is inconvenient, the Roman, which harmonizes much better with our own than that of any modern nation. We could offer various other remarks on medical barbarisms; but forbear on account of the occasion, and ought to apologize for so occupying thus much of the Quarterly Summary. The *pleximeter* is not described by the reviewer, on the ground that "it is in every body's hands." Whether the instrument be the same which is in our hands, we are thus rendered unable to determine. The latter consists of a circular piece of wood, about two inches wide, and fastened to a small handle, placed obliquely. The flat piece is applied against the thorax or other part to be examined, for the purpose of being tapped on by the fingers.

Dr PIORRY's opinions may be divided into two classes; those which he considers himself as having demonstrated by observation, and those which he believes himself authorized to infer from analogy, and from the nature of the pro-

cess described, we give a sketch, compiled, with some care, from the review in the "Nouvelle Bibliothèque Médicale."

In hydrothorax, the fluid is naturally found in the lowest portion of the thorax, in whatever position the patient is placed for examination. When not of sufficient volume to fill the whole pleura, the existence of the fluid may easily be detected by attention to this principle. When the patient sits erect, the lower part of his thorax emits, on percussion, a sound obviously duller than natural; and this extends, in every direction, as high as a horizontal line easily ascertainable by this mode of inquiry. When the patient lies on his back, after a few moments of rest, the whole anterior edge of the thorax, which is now uppermost, recovers its natural sonorous state, and sounds hollow when struck; while the duller sound is produced all along the posterior part. Again, a corresponding change is produced by placing the patient on his face, or on either side; the part left uppermost being found sonorous, and the lowermost, to which the fluid has subsided, becoming otherwise. In every instance, the line of demarcation is distinct and horizontal. Now, in pneumonia, in which the substance of the lungs is rendered thick and hard, a similar dull sound is produced; but we have here no change in the position of the part which yields it, and no horizontal line of demarcation between the healthy and the diseased.

Where the dropsy fills the whole pleura, and the duller sound is heard over the whole surface of the thorax, on one side, it is extremely improbable that the disease can be mistaken for pneumonia; because the latter disease has hardly ever been known to affect the whole of a lung. This has however sometimes occurred; leaving a deficiency in this part of the diagnosis.

The sound produced where there exists a pleuritic effusion of moderate depth, is not what is called absolutely dead; but forms a tolerable medium between that emitted by tapping over the heart, and that exhibited by sound lungs subjected to the same trial. When the quantity of liquid at liberty in the thorax is very small, it must be sought for, not only at the lower or abdominal extremity of the cavity, but at the posterior part, next the vertebral column.

Inflammation of the lungs, then, almost always presents inequalities of outlines, by which it may be distinguished from pleural effusion. Of apoplexy of the lungs, (the local effusion described by LAENNEC), the same remark holds good. A dead sound obtained in the regions immediately above or below the clavicles furnishes a *very constant* sign of tubercles. A gradation of sound a little less clear on one side of the thorax, when observed in the situation just mentioned, affords a suspicion of numerous miliary tubercles. A sound less clear over the whole of one side than on the other, or any very obvious differences of sound between different points in the same side, if, at the same time, the symptoms of pneumonia are wholly wanting, "may produce a belief" of the existence of tubercles scattered generally throughout the lungs, and separated by portions of sound lung.

When there exist large, empty excavations, situated near the surface of the lung, surrounded by tuberculous or hepatised portions, and, around these again, by healthy lung, the percussion yields a tympanitic sound, of loudness proportioned to the dimensions of the cavity. This sound is not detected in very small excavations. If the cavity be half filled with fluids, and there exist open communications with the bronchia, there will be a "*humoral* sound," capable of being easily distinguished. The same phenomenon is produced when the cavity is placed at a considerable depth.

For the purpose of studying diseases of the heart, percussion with the plexi-meter should be combined with feeling and the use of the stethoscope. The author here gives some opinions which the reviewer treats as hypothetical, or, at least, as requiring further observations to test their correctness. If the dull sound produced by percussion over the heart be heard for a space exceeding two inches, [in diameter?] and the pulsations of the organ can be felt over the whole surface, Dr PIORRY considers the heart as enlarged at its point and anterior surface. If the pericardium contain much liquid, the dead sound is very distinct on the left side. Tumours of the aneurismal character, it is thought, can be detect-

ed; and if covered by a thin stratum of lung, a slight tap will produce the hollow sound; while a harder blow sounds dull.

In the *abdomen*, our reviewer expresses himself dissatisfied with Mr PIORRY's results in regard to affections of the peritoneum lining the muscles, in tumours, and particularly in hydatids. These indications are omitted in his analysis. In effusion in the cavity at large, the same observations may be made which we have recorded of the thorax when under the same circumstances. The stomach yields a very clear "son stomacal"; and the intestines a sound intermediate between the latter and that of the liver; which last yields "son jecoral." To the remarks which relate to position should be added the consideration, that when the quantity of fluid in the abdomen is very small, it collects entirely within the cavity of the pelvis, from which it must be made to issue by elevating the patient's hips; when, according to the part placed lowest, it may be detected beside the vertebræ, on either side, or along the front of the belly.

Encysted dropsies yield a dead sound, far more dull than that of fluid rolling at large among the intestines. It may also be recollected, that the sound of the latter is more exclusively dull at the lower part; and yields some sonorous effect when struck at the upper part where intestines may be supposed to be floating partly immersed within it.

We candidly confess we are unable to understand the description given in the review of the situation and extent of the jecoral sound. We should prefer searching for it by our knowledge of the situation of the liver. Indications of an obvious nature must be expected, when the liver is greatly increased or reduced in size. The reviewer thinks that small enlargements or diminutions could not be discovered in this way. Dr. P. conceives that he can distinguish the spleen, but there is great uncertainty about this organ. By thrusting the front of the abdomen upwards, an assistant can elevate the liver and stomach, and with them the spleen; and this leaves the kidneys uncovered, in which Dr P. proposes to discover enlargement by means of his instrument. We confess we should despair of success in such an attempt.

In cases of peritonitis, Mr PIORRY recommends the employment of a very broad pleximeter, to be struck with great precaution, from a fear of bruising the inflamed membrane.

Percussion will demonstrate the distended bladder by its dull sound; if, at the same time, the patient be placed in different attitudes, and the dulness be found, notwithstanding, to continue in the same place.

The affections of the *uterus* he postpones for future inquiry; and a field is here opened for other experimenters.

IV. THERAPEUTICS, MATERIA MEDICA, AND THE PRACTICE OF MEDICINE.

12. *The Conservation and Reproduction of the Medicinal Leech.*—We gladly borrow an article, having the above title, from the July number of the London Medical Repository, in the confident belief that it cannot fail to interest our readers at this present time when the practice of free and frequent leeching is becoming so general in this city and other parts of the United States, in not only the phlegmasiæ, nosologically so called, but in all febrile diseases.

M. CHATELAIN has published two treatises on this subject *(Annales de la Méd. Physiol. Jan.* 1827, *et Journal de Physiologie Experimentale*, tome vii. 1827). His observations confirm the supposition that the leech is androgynous, and that the generative process is performed by reciprocal and simultaneous impregnation. One leech is capable of producing three capsules *(cocons)*, but on an average, one capsule only is obtained from each individual, and nine leeches from each capsule. The author has observed where a plurality of capsules oc-

curred, that they were formed at intervals. The young escape from the capsule within twenty-six or twenty-eight days.

Contrary to the opinion of Dr PALLAS (*Journ. de Pharmacie, Juin*, 1827), M. CHATELAIN thinks sanguisuction is unfavourable to the fecundity of the leech. The plethora thus occasioned generally caused a mortality of one-third of his leeches in the space of two or three months, notwithstanding frequent change of water and diversity of situation. In order to effect disgorgement of blood, the leeches were usually subjected for four or five minutes to a solution of one part of common salt in ten of water. For their reception during the season which M. CHATELAIN found in Provence to extend from the latter part of July to the middle of September, stone pans or jars, each containing a sufficient bed of moist clay, were arranged in the open air, so as to be fully exposed to the sun. Two fine leeches were placed in each jar, which was then covered with a cloth of open texture to prevent escape without excluding all light. With the same view, M. CHATELAIN was accustomed to draw a brush, dipped in equal parts of sulphuric acid and water, round the inside of the jar, about four or five inches above the clay. The jars were visited daily, and care taken to keep the clay moist.

It is remarkable that the pairs could never be detected in the act of copulation. This was supposed to be accomplished within the burrows which these animals made in the clay.

The Preservation of Leeches.—M. PFEUFER, physician to the hospital of Bamberg, recommends the use of a wooden tub, furnished with a cock and a long funnel, for the purpose of renewing the water, which should be done with as little disturbance as possible. At the bottom of the tub he would have some mud from the pool whence the leeches were taken, with one or more roots of the calamus aromaticus or of the iris florentina. This was the method adopted by M. CHATELAIN, and is followed in most of the hospitals in France.

M. SCHUTZ takes some ounces of fresh powdered charcoal, which he washes sufficiently with water; on this powder he pours a solution of twenty grains of sugar of milk in warm water. In this mixture he places his leeches, keeping them in a temperature between 50 and 55° Fahr. The water, as well as the solution of the sugar of milk is renewed every fortnight; great care being taken that the water be not too cold, and that the vessel be not much shaken.

M. HAMPE uses small tubs charred on the inside, covered only with gauze. At the bottom is placed sand, and upon this some moss. River water only is employed, to which he adds a few lumps of charcoal. In summer the water is changed weekly, by means of a hole pierced immediately above the layer of sand. In winter it is enough to change the water once in six weeks, preventing the access of cold. This method is much recommended.

M. VOGET also advises us to add to the water some of the soil to which the leeches have been accustomed; but the peculiarity of his method is the addition of the oxide of iron.—*Bulletin des Sci. Méd. Avril*, 1820.

For farther particulars on this subject, as well as on the manner of applying leeches for the abstraction of blood from the skin of the patient, we refer our readers to the Review of *Derheims on Leeches* in our first volume.

13. *Iodine in Gout.*—Doctor GENDRIN, the able author of *L'Histoire Anatomique des Inflammations*, speaks in high terms of the curative powers of the iodine in gouty affections. He first employed it externally for the discussion of arthritic tumours. Encouraged by his success, he afterwards prescribed it in acute and violent gout with equally fortunate results. Several patients were completely cured by the medicine. In two only was the treatment difficult. The continued use of the iodine for two or three months after the complete cure of a paroxysm prevented its return.

Before giving in our adhesion to this new practice, we would like to know precisely the condition of the digestive passage as regards the sensations of heat, tenderness, spasm, or pain, the irritability of the stomach, the extent of the sympathy of the sanguiferous system, whether the attacks were chiefly

articular or gastric. These points determined, we ought next to be apprised of the dose by which to begin the treatment.

14. *Bloodletting in Peripneumonia in Children.*—Dr CUMING, assistant physician to the "Institution for the diseases of Children" in Dublin, gives the following directions in regard to the quantity of blood and the manner of abstracting it in the cases under consideration:—

"In a child about two years of age, from three to four ounces may be abstracted; and when the age is above four, about five, six, or eight ounces may be drawn, according to circumstances. In an infant under six months, though general bloodletting may often be required, the application of three or four leeches to the back of the hand or foot will for the most part answer the purpose where a vein, which is frequently the case, cannot be found. It is better to apply the leeches to the hand or foot than to the thorax, for, when they are applied in the latter situation, it is difficult to stop the bleeding after they have fallen off; and instances have occurred in which a fatal hemorrhage has been the consequence of a continual oozing from four or five leech bites. When they are applied to the hands or feet, the bleeding can be easily stopped by placing compresses of lint over the bites, and securing them, as after the operation of V. S. by a bandage. As far as my observation goes, leeches applied to the extremities are nearly as efficacious in removing local inflammation in infants, as when applied in the vicinity of the parts affected. They seem to produce the effect of a general bloodletting: as the face and lips become pale, the pulse faulters, and syncope occasionally takes place followed by vomiting. These effects are apt to be produced when general venesection is carried to a considerable extent, and sometimes a state of nervous agitation and general commotion is induced, which, if not speedily removed, may terminate in death. The best remedies in a case of this kind are the horizontal position, cool air, and a drop or two of the tincture of opium."

When we hear, as in this instance, of an experienced physician recommending three or four leeches to the back of the hand or foot as an adequate means of drawing blood from an infant under six months attacked with peripneumonia, we on this side of the Atlantic are bound not to deride his practice, but rather to admit that the European is a much more formidable animal than the American leech—the more especially since instances are said to have occurred of fatal hemorrhage from the application of three or four of the former to the thorax of infantile subjects; an occurrence never thought of among us. Our leeches are not such desperate sanguisuckers, otherwise we could hardly dare to apply, as was done by the writer of this article, ten leeches to the neck of an infant eight days old, for bronchitis approaching in some of its symptoms to croup. The warm bath and antimonials had been used without effect. At times there seemed to be a complete suspension of respiration, and from all appearance the little sufferer could not have lived two hours longer unless decided relief were afforded. The leeches procured this relief, and, from a few hours after their application until the present time, the child has been perfectly healthy. Within these few days we directed the application of twelve leeches over the epigastrium of an infant seven weeks old for violent pertussis. No hemorrhage was feared or ensued after the bites of these animals; nor was there any notable debility or languor induced by the loss of blood which they had abstracted. They were, however, vigorous and drew well.

15. *Cases in which the Moxa was successfully applied.*—Such is the heading of a paper by R. WADE, member of the Royal College of Surgeons, and inserted in the Med. and Phys. Journ. for June 1828. In the case of epilepsy cured by this means, the moxa was applied at first in three places, viz. one over the junction of the occiput with the atlas, and one to each side of the neck, and subsequently in two places daily for a period of six weeks; and every third or fourth day during three months, with an interval between these two periods of six

weeks, during which time the patient (a female aged seventeen years) was in the country.

The case of paralysis was of a female aged fifty-seven. The muscles of the right side were quite useless, and scarcely any indications of returning sensation were observed. Other remedies failing, a moxa was burnt slowly over the deltoid, and another over the biceps; a slight redness of the skin being produced by their combustion. On the very next day there was more sensation in the arm, and the patient was able to lift it three or four inches from her side, although on the preceding day she had not the slightest power over the muscles. Four moxas were applied on this day—one to the deltoid and three to the glutos. The application of the remedy was continued to different muscles of the body for about five weeks, with the effect of enabling the patient to walk with the assistance of crutches. The use of the flesh brush, and as much exercise of the muscles as could be borne without fatigue, completed the cure.

16. *Bromine and the Hydro-bromate of Potassa in Scrofula and Goitre.*—We read in the *Ephémérides Médicales de Montpellier* for May an account of the medical properties of these substances, by Mr Pourche. Bromine, regarded by some of the most authoritative chemists as a simple body, is found in sea-water. It is fluid at the common temperature of the atmosphere, and even at 8° F. below zero. The first case was of scrofulous tumours of a large size on the neck of a young woman aged twenty-two years. M. P. prescribed six drops of bromine in three ounces of water—to be taken in the course of the day in three doses. On the following day four drops were directed in the same quantity of water. After the lapse of ten days the daily allowance was increased by four drops. From this time the patient experienced, after taking a dose of the medicine, but only during five or six minutes, some flushing in the face, a slight cephalalgia, and a dryness of the throat and thirst. The quantity of the bromine was augmented in a month from its first administration to thirty drops daily. Cataplasms of crumbs of bread moistened with a small quantity of the bromine were likewise applied to the tumours. Perseverance in this course for three months was productive of a removal of the tumours, with the exception of a hard and large tubercle, the resolution of which is not hoped for by the physician.

In another case of scrofula marked by tubercles of the neck and ophthalmia of one eye, the other having been lost by scrofulous inflammation, M. P. prescribed the hydro-bromate of potassa internally in the form of pills, and externally as a pommade. The patient took at first two pills of a grain each daily, and frictions were exercised on the part three times during the same period with a pommade composed of one drachm of hydro-bromate of potassa and an ounce of lard. On the sixth day of the treatment four pills were taken, and on the eighteenth, six, and on the thirty-second, eight. This course, persevered in for three months, accomplished a complete removal of all the scrofulous symptoms. The only evidence of excitation from the medicine was a slight increase of temperature for two or three minutes after the ingestion of the pills.

The third case was of chronic otorrhœa, with a hard tuberculous swelling of the epididymis of each side, the latter consequent on gonorrhœa, but for which mercury had been administered without effect. The patient had suffered under an attack of tubercular phthisis of which he had been *cured* by four moxas and the use of preparations of gold. The hydro-bromate of potassa was, on the present occasion, prescribed internally in the form of pills, and the bromine applied externally to the parts. For the first ten days, the patient took four pills of the hydro-bromate of potassa, of one grain each; during the ten days following, six; and for two months, eight pills daily. Cataplasms, moistened with a solution of bromine, were applied to the scrotum, and injections of bromine, thirty drops to six ounces of water, were thrown into the external meatuses. This treatment was continued for a period of three months, after which both the otorrhœa and the indurated epididymes had entirely disappeared.

The fourth case was of goitre, the treatment of which by the bromine was

completely successful. The author thinks that this new agent is entitled to a higher rank in the class of remedial agents than iodine.

17. *Intermittent Facial Neuralgia cured by Sulphate of Quinia, used according to the Endermic Method.*—Dr CAUCANAS relates the history of this case in the *Journal Universel des Sciences Médicales* for May. The subject was a lady, aged thirty years, who, after exposure to a cold and moist air, was seized with acute pain of the left ear, which soon extended over the whole left side of the face. Various remedies were put in requisition to allay and remove this malady which made its attack every evening. Repeated leeching the part —opiates—the carbonate of iron were all tried in vain. During the administration of this last mentioned article the stomach became excessively irritable. The periodicity of the disease seemed to indicate the use of the quinia, which could not however be given in the usual manner on account of the state of the stomach. On a vesicated surface on the arm, therefore, fifteen grains of the sulphate of quinia, mixed with some cerate, were applied. The day was passed tranquilly—no paroxysm during the night, and seven hours of sleep obtained. A similar application on the following day gave a night of entire tranquillity. Two more days of treatment were sufficient for an entire cure.

18. *The Warm Bath in Intermittent Fevers.*—Dr GASSAUD of Calvi has published, in the *Nouv. Bib. Méd.* for June, some observations on the advantages derived from the use of the warm bath in intermittent fevers. It is, as he justly remarks, a very ancient remedy, sanctioned by the recommendation of Hippocrates himself. Of the cases which Dr G. adduces as illustrative of its efficacy some are not of a very confirmatory nature, since the cure would seem to have been finally brought about by very reduced diet and abstinence from all tonics and stimuli, and even in two instances by the application of leeches to the epigastrium. He recommends the patient to be immersed in a bath of blood heat on the day of the fit, and to remain in the water as long as his strength will allow.

19. *Cases of the Poisonous Operation of Iodine.*—One of these was of a lady, twenty-six years of age, of a nervous and irritable habit, who, wishing to put a period to her existence, swallowed two drams and a half of the tincture of iodine. The immediate effects experienced were dryness and burning of the throat, lancinating pains of the stomach, nausea, efforts at vomiting; the face was flushed, eyes watery, pulse small and contracted; the pains of the epigastrium were exasperated by the slightest pressure; convulsive pains of the upper limbs.

Vomiting was excited by glassfuls of warm water taken at intervals; the ejected matter was yellow and had the odour of iodine. An anodyne draught was then administered and an opiate plaster applied to the epigastrium. Small enemata of a decoction of poppy heads were also used at the same time. All the unpleasant consequences were soon removed and yielded to a refreshing sleep.

Dr GENDRIN, editor of the *Journal Géneral*, in commenting on this case, tells us that of all the preparations of iodine the tincture is the one which can be least tolerated in large doses by the patient. We may, however, by gradual progression in the course of a few days, carry the quantity as high as one dram to a dram and a half of the tincture in the twenty-four hours. The solution of the hydriodate of potass is much more readily borne, and can after a time be taken in very large doses. Dr G. was giving at the time he wrote (in June) an ounce of this preparation daily to a lady labouring under very painful chronic gout. On the third day of the administration of the medicine the pain was diminished: on the eighth it had ceased entirely, although for two months preceding it had persisted uninterruptedly; the swelling of the joint is rapidly diminishing.

Another case related in the Journal above mentioned is of M. A. CHEVALLIER, who suffered from violent colics on two different occasions by exposure to the vapours of iodine.

M. RASPAIL, on the other hand, cites his own personal experience of the im-

punity with which the vapour of iodine may be inhaled for some hours in a room filled with it.

20. *Melancholy with Aphonia cured by the Actual Cautery.*—Dr Rossi relates, in the *Repertorio di Torino* for January last, the case of a man thirty years of age, of a robust constitution and sanguine temperament, affected with profound melancholy and complete aphonia. Various methods of cure had been resorted to in vain: and the cauterisation of the vertex by iron of a white heat was finally determined on. The action of this agent extended to the bones. The patient was better on the very next day after the application, and could answer questions in a low voice. In proportion as suppuration was established the intellectual faculties were restored. Purgatives were then had recourse to; the posterior part of the neck was covered with blisters, whilst the anterior was rubbed with ammoniacal liniments. The infusion of arnica was given internally with the addition of camphorated ether. Ammonia was inhaled by the patient from time to time. At the end of two months he had recovered his health.

We should be tempted, notwithstanding the caption of this article, to ask by what means the disease in this subject was finally removed. If cauterisation were relied on, why so prodigal in medication of other kinds.

21. *Ammonia in Drunkenness.*—Dr Vatteteau communicates some cases in the *Journal Universel* for April, in which the stupor and other unpleasant and even alarming symptoms of drunkenness were speedily removed by the ingestion at short intervals (ten minutes) of liquid ammonia, in doses of six to twelve drops, mixed with sugar and water or some such simple drink. Occasionally he directed the volatile alkali to be applied to the nose of the drunken person and to be inhaled by him. Sulphuric ether was also administered with equal success in doses of twelve to fifteen drops in a suitable vehicle. Two or three doses of the ether, making in all thirty to forty drops, usually suffice to restore the drunken man to the use of his senses and intellect; and to enable him to move about with readiness.

22. *Erysipelas cured by Cauterisation.*—A young soldier of the royal guard had a spreading erysipelas of the lateral, anterior and posterior parts of the chest, consequent on the application of caustic potass to the soft central part of a swelling on the right axilla. The eschar was excised, but erysipelas supervened, and was only arrested by slightly touching the affected surfaces in many points with iron of a brown heat.

23. *Hydrochloruret of Lime as a Remedy in Fever and Dysentery.*—Dr Reid gives several cases (*in the Transactions of the Association of the Fellows and Licentiates of the King and Queen's College of Physicians in Ireland*) of the good effects of this medicine in the above mentioned diseases. In one instance, in which the patient was recovering from fever, a relapse was brought on by his friend stupidly though with kind intention giving him some wine to drink. "2d April.—In the night he complained of most violent pain of the head. He lies nearly insensible. Low muttering delirium. Tongue deep yellow, dry and furred; great subsultus.

Half an ounce of the mixture, with hydrochloruret of lime and tincture of Columbo, was directed to be given to him every second hour.

3d. Much better in all respects; general warm perspiration, not profuse, but still some delirium and subsultus.—Continue medicines.

4th. Tongue cleaning, subsultus gone. No delirium.

5th. Still continues to improve. Tongue dry, and he cannot put it outside his teeth, though he apparently makes considerable effort when desired to do so. I directed a blister to his neck, and to continue the medicine as before.

6th. Much fetid discharge from the blister. Continues to improve; can now

freely put out his tongue when desired. It is now moist and clean at the edges. He has some appetite to day.

7th. Still improving. The diaphoretic mixture was now substituted for that with the hydrochloruret.

12th. Up and dressed, sitting on his bedside. Well.

This was one of the most remarkable cases (says Dr REID) I have witnessed of the efficacy of the hydrochloruret; whether it be considered with reference to the severity of the disease, or to the rapidity of the patient's recovery."

A case is related by Dr R. exhibiting the influence of the hydrochloruret of lime in controlling disease of the pulmonary organs resulting as he says from febrile excitement.

In dysentery, the hydrochloruret was used both by the mouth and with enemata. The latter was found a very effectual means of correcting the fetor of the stools which at times amounted to an intolerable stench.

The mixture directed by Dr REID was ten grains of the hydrochloruret of lime with two drachms of the tincture of Columbo, in four ounces of water and two drachms of syrup; half an ounce to be taken every hour or two.

24. *Treatment of the Epidemic Dysentery which prevailed in Dublin in* 1825.—Dr O'BRIEN, in his paper on this disease contained in the Transactions of the College of Physicians in Ireland, gives the following as the plan of treatment which he adopted and which appeared on the whole to prove most successful. "The patient, if young and in the first stage of the disease, was immediately blooded to the extent of twelve or fourteen ounces; he was then placed in a warm bath at a pretty high temperature, in which he was retained for fifteen or twenty minutes, if possible, but the harassing necessities of the disease seldom permitted so long an interval of quiescence. He was then, after a vigorous friction of the whole body, but particularly of the abdomen, rubbed with camphorated oil, put into bed, having been previously swathed with a flannel roller, drawn tightly round the abdomen and loins, in order to support the bowels, a measure which is always attended with comfort and alleviation of pain to persons labouring under this disease. After this a small bolus, or pills containing ten grains of calomel, one or sometimes two grains of opium, and occasionally combined with antimonial powder, were administered, and repeated after an interval of eight hours, if the urgency of pain rendered such repetition necessary, for the anodyne effects of opium scarcely extend beyond this period. As patients are generally conveyed to the hospital at a late hour in the day, the measures just described occupied the first evening and night after admission. On the morrow a purgative draught, consisting sometimes of castor oil, tincture of rhubarb, and tincture of opium; sometimes infusion of senna and salts, and sometimes the neutral salts alone. The author has not found from experience that castor oil possessed more irritating qualities than the other purgatives above mentioned, and thinks that the prejudices which some practitioners entertain against it are altogether groundless; but all apprehension on this subject may be removed by the addition of tincture of opium, which the author thinks will even promote its purgative virtue in the disease before us. If the symptoms still continued urgent, and the pulse gave indication of the presence of inflammatory and febrile action, the bleeding and warm bath were repeated, and the calomel and opium continued as above, alternated however by purgatives until the disease yielded, or until ptyalism, or, at least, soreness of the gums was effected. When this latter effect was produced, which in some instances was accomplished with considerable difficulty, the mercurial part of the medicine was discontinued, and the opium alone exhibited, accompanied as before by the alternate use of purgatives. In many instances the Dover's powder was substituted for opium and achieved the cure with equal success, and indeed this modification of opium gained confidence in proportion to the frequency of its employment, and cannot be too highly commended in this disease."

Of the patients under Dr O'BRIEN's care, about one in four died, and he con-

siders this number as a fair average of the ordinary mortality from dysentery in the Dublin Hospitals.

The duration of the disease when it terminated favourably varied from two to six weeks. The discharges from the bowels were of a dark brown colour, exhibiting insulated patches of blood floating on the surface. The seat of pain was, generally, the prominent part of the hypogastric region, a little below the umbilicus. The colour of the skin was in every instance altered to a darker hue, assuming, as the complexion varied, either a leaden or a copper tinge; and in general it affected the touch with the sensation of roughness and even corrugation. The eyes were anxious, the brows wrinkled, the countenance dejected, sunk, pale, and cold; the features sharp and pointed.

The post mortem appearances in twelve cases were as follows: "The liver was found diseased in six; the spleen in three; small intestine (chiefly ileum) inflamed or ulcerated on its mucous surface in eight; great intestine diseased in twelve; gangrenous in one; much contracted in two; ulcerated and inflamed in all; colon and rectum parts most diseased."

It must be very difficult after the evidences thus furnished of the seat of the malady and of the tendency to disorganization of the parts, together with the symptoms, for a physician of any observation to withhold the use of the lancet and topical means of depletion by leeches, and, where the abdomen is not too tender, by cups. Dr O'Brien does not seem to include topical bleeding as one of the curative agencies in dysentery: he is silent also as to the efficacy of enemata, whether of cold water in the earlier stages, or of mucilaginous fluids and opium combined in the more advanced periods of the disease. And yet, if we were required to choose, we should have no hesitation, arguing from the sporadic cases of dysentery which annually come under our notice, to give the preference to venesection general and local, diluents and opiates by the mouth and enemata, over any and every other kind of medication by vomiting, purging, sweating and blistering. Even if purging were thought admissible we should have great misgivings about allowing our patients to take purgative tinctures, as of rhubarb, or any preparation of senna, which however prepared is so apt to excite violent griping.

25. *Suppression of the Urinary Secretion for more than seven weeks, cured.*—One of the German journals contains the history of a case of this nature recorded by Dr De Racum of Riga. The subject of it was a child twelve years of age, in whom the secretion of urine was suppressed without occasioning any derangement of the other functions, or without any auxiliary or supplemental process of secretion being carried on during the period above mentioned. The introduction of a silver catheter into the bladder was not followed by the discharge of a single drop of urine. After attempting various other methods ineffectually Dr De Racum had recourse to the following preparation, which restored the renal secretion on the third day. Purified oil of amber, two drams; Venice turpentine, ten drams; balsam copaiva, one ounce. Thirty drops to be taken thrice a day in emulsion of almonds. In addition to this he directed friction with oil of turpentine twice a day on the lumbar region; and a regimen vegetable and diuretic. The author assures us that the above remedy is equally efficacious in gravel.

26. *Good Effects of Cold Affusion.*—In the fourth number for July of Dr Drake's *Western Journal of the Medical and Physical Sciences*, we find related a case of *ischuria* successfully treated by the cold affusion. The patient had at the time a dysentery. He was placed in a tub of tepid water, and bled freely from the arm, but without any relief to the suppression of urine now of forty-eight hours duration. Dr Lewis Campbell, his physician, who is also the narrator of the case, still allowing him to remain in the bath, took a half gallon pitcherful of cold spring water, and poured it in a continued stream on the region of the bladder and pubis. Before the third pitcher had been emptied, the urine broke forth in a full and free stream. The same remedy was had recourse

to on the four following days—other treatment adapted to the dysentery being employed during this period. After this the convalescence was speedy and uninterrupted.

The next is a case of *poisoning by opium* treated with success, by the affusion of cold water, by Dr JOHN F. BROWN of Tennessee. The patient had swallowed opium at two different times the preceding evening: the first piece made him vomit, and he repeated the portion, which, from his description, must, Dr BROWN thinks, have weighed two drams. Fourteen hours elapsed from the time of his taking the opium until the physician arrived. All attempts to provoke vomiting by the forced ingestion of tartar emetic and sulphate of zinc and tickling the fauces proving ineffectual, it was resolved by Dr BROWN, in consultation with Drs YOUNG and SNIDER, to have recourse to cold affusion. The patient was accordingly laid on the floor, and a large vessel of cold water poured on him, commencing with the face and directing it to the breast. The second vessel produced some struggles, and so soon as the third was beginning to be poured on him, he suddenly raised himself on his seat, and said "I cannot stand every thing." The cold affusion was continued, and so soon as sensation was restored, vomiting was produced by titillating the fauces. Strong coffee was next freely given. In about two hours the emetic medicines produced catharsis. Frictions and moderate exercise, with some other gentle remedies, were then recommended.

In the evening, when Dr BROWN repeated his visit, he found his patient cheerful and apparently free from the effects of the opium.

27. *Obstinate Constipation relieved by Injections per Anum.*—The August number of the London Medical and Surgical Journal (formerly the London Medical Repository) contains a short paper by Dr EPPS, one of the editors, the chief object of which is to recommend a perseverance in the employment of enemata to relieve obstinate constipation. He gives the case of a gentleman who had taken calomel to the extent of producing salivation, saline draughts, manna and rhubarb mixtures, and castor oil, with the view of opening his bowels, but all to no purpose. The colon and cæcum were found by Dr EPPS distended with feculent matter, which, together with the medicines that had been taken, had induced so much irritation that the least touch on the abdomen, in the situation of the cæcum, induced the most pungent pain. Leeches were ordered to be applied over the parts most painful, and an injection of castor oil to be given, an injunction being added, that should it not operate, common barley water injections were to be exhibited every hour until an effect was produced. The castor oil enema did not operate, and it was not until sixteen injections in the whole had been administered that the bowels were opened. In a week or two the patient perfectly recovered. Dr EPPS adverts to the good effects of this remedy in children in whom the most unpleasant symptoms have been relieved by it.

V. MEDICAL JURISPRUDENCE.

28. *To determine, approximatively, a long time after Death, whether the Individual died from Poison, and to ascertain the nature of the Poisonous Substance.*—Such are the questions to solve which have been the aim of Professor ORFILA and Dr O. A. LESEUR, in their memoir on the subject. Our knowledge of it is chiefly derived from the initial article in the *Journal de Chimie Médicale, de Pharmacie et de Toxicologie, Juin*, 1828; and from this we propose drawing on the present occasion.

The conclusions of these learned medical jurists rest chiefly on the two following kinds of experiments:—

1. Mineral and vegetable poisons in varying doses were dissolved in a pint of water, and mixed with animal matter; the mixture was exposed to the open air in

vessels with a large opening, a portion of the fluid contents being analysed every eight or ten days.

2. The same poisons mixed with albumen, gelatin, flesh, &c. were enclosed in stomachs and intestines, which latter again were put in deal boxes, well closed and buried at the depth of two and a half feet. After the lapse of several months these boxes were dug up, and their contents analysed.

On the other hand, again, human bodies, buried in deal coffins at the depth of four feet under ground, have been disinterred one, six, ten, and even seventeen months after inhumation, in order to ascertain as near as may be the epoch at which vestiges of the digestive canal are still discoverable, and to exhibit the presence of a poisonous substance which may have been introduced into the stomach or intestines during life. This last inquiry, which is to be the subject of a work by MM. Orfila and Leseur, on *juridical exhumations*, has led them to the discovery that for many months, and even for years after death, when a soft part can be no longer recognised, there is found on each side of the vertebral column, in the abdominal region, a kind of brownish *coom*, which is evidently the remains of the digestive canal, and in which a portion of the poison can have remained either altered or in its natural state.

The poisons which constitute the subject of the present memoir are the sulphuric and nitric acids, and arsenious acid (white arsenic), corrosive sublimate, tartar emetic, sugar of lead, proto-hydrochlorate of tin, sulphate of copper (blue vitriol), nitrate of silver (lunar caustic), hydrochlorate of gold, acetate of morphia, hydrochlorate of brucia, acetate of strychnia, opium, and cantharides. These were examined in succession, as follows:—

Sulphuric acid (oil of vitriol).—It is possible to be assured of the presence of *concentrated* sulphuric acid several months and even years after its admixture with animal matters; all that is necessary is to treat the mass in which its existence is suspected, with tincture of turnsol, or hydrochlorate of barytes, or metallic mercury. If the sulphuric acid have been much *diluted* and mixed with substances, which by putrefaction disengage ammonia largely, it is saturated by this alkali to such an extent that there is none or hardly any of it free at the end of some months. In this case we are no longer authorised to conclude that there has been poisoning by means of sulphuric acid; at most we are at liberty to infer, supposing the sulphate of ammonia to have been obtained in a crystallised and well marked form, some *probability* of poisoning, as this sulphate does not *commonly* constitute a part of the alimentary matter, nor of those composing the digestive canal. The best means of ascertaining the presence of a small quantity of *free* sulphuric acid in the cases in question would be to treat the liquid at a common temperature with *pure* sub-carbonate of lime, freed from all sulphates: there will be formed, in the course of a few seconds, sulphate of lime if there be free acid, whereas none will be produced if the liquor only contains saturated sulphate of ammonia, since this salt is not decomposed at a low temperature by the subcarbonate of lime until after the expiration of a certain time.

Nitric acid (aqua fortis).—*Concentrated* nitric acid may be detected many months after its mixture with animal matters, and even when putrefaction is at its height. For this purpose it is sufficient to treat the liquor by potassa, then to evaporate it to dryness and to throw a portion of the product on live coals; another part mixed with metallic copper and sulphuric acid diluted with a very small quantity of water will furnish vapour of an orange yellow colour. If the nitric acid have been considerably *diluted* and employed in small quantity, the ammonia, which results from the putrefaction of the animal matters, being more than sufficient to saturate all the acid, we can at most merely prove the fact of the presence of nitrate of ammonia, which does not *necessarily* suppose that there had been poisoning by means of the nitric acid, since the nitrate just mentioned might in truth be formed from all the parts during the putrefaction of the animal matter.

Arsenious acid (white arsenic).—We have it in our power to show the presence of arsenious acid which has been mixed with animal matters, after the expiration even of several years. We must, however, in many cases, in order to

succeed in this, free it from the greater portion of these matters by evaporating to dryness the liquid which contains the poison, and agitating for some minutes the product of the evaporation with boiling distilled water; the solution then exhibits the same characters with hydrosulphuric acid (sulphuretted hydrogen) which the pure arsenious acid would do. We cannot too carefully direct attention to the fact, that, by its combination with animal matters, the arsenious acid thus dissolved is so far disguised as *not to become yellow* even when treated with hydrosulphuric acid, but it is sufficient to have recourse to the simple method above indicated to render its presence fully evident. If the writers, who have proposed more complicated processes, have not detected arsenious acid in the liquids ejected by vomiting and in the fluid contents of the stomach and intestines, it is because they have not pursued the course indicated above, and more especially they have neglected uniting these liquids with hydrosulphuric acid and a drop of hydrochloric (muriatic) acid, whereas the deuto-sulphate of ammoniacal copper is an uncertain reagent, as already shown in the *Leçons de Médicine Legale*, 2d edition, tome iii. p. 112.

If the arsenious acid or white arsenic have been given in a solid state it will not be impossible even a length of time after inhumation to discover here and there grains, which being taken up on the point of a penknife will exhibit all the characters of the arsenical poison.

We cannot doubt but that the arsenious acid is after a time transformed, and, in proportion as ammonia is generated, into an *arsenite of ammonia*, which is much more soluble than the arsenious acid: hence it may happen that after the lapse of some years we are unable to demonstrate the presence of arsenious acid, where it would have been very easy to have detected it some months after inhumation, because this acid before solid and granular became so soluble as to have transuded through the pores of the coffin, or had flowed off through the holes which are often found on the lower surface of these boxes, when putrefaction has made rapid progress.

Though the arsenious acid employed in large quantities stops the putrefaction of animal matters, this is not the case when used in minor proportions.

Corrosive sublimate.—Corrosive sublimate dissolved in water is decomposed with considerable rapidity so as to render it impossible for us to detect its presence in the liquid matters after some days, except by means of a plate of gold covered in spiral fashion by another of tin, aided by the action of hydrochloric acid. There is more of the sublimate decomposed in proportion to the large quantity of animal matters made use of. It does not appear, however, that these matters can decompose all the corrosive sublimate, since by the aid of a plate of gold the experimenters succeeded at the expiration of *several hours* in procuring an atom of metallic mercury from a solution of six grains of the sublimate mixed with a *large quantity* of animal substances. In all cases we can, by treating, by means of heat and potassa, the animal matters which decompose the corrosive sublimate, obtain metallic mercury, even many years after the sublimate has acted on them. Now the presence of this metal, though it may not prove the existence of corrosive sublimate, indicates at least that of a mercurial preparation.

Tartrate of potassa and antimony.—Tartar emetic mixed with animal substances is decomposed after some days, so that the tartaric acid is destroyed and the oxide of antimony precipitated. Whence it results that it is impossible to exhibit this salt in treating it by the reagents usually employed to detect it; but we can obtain metallic antimony from solid matters even after the expiration of many months. The alteration in question is rather the result of the action of water and air on this salt than of animal substances; for experience proves that a solution of three grains of tartar emetic in a pint and a half of distilled water exposed to the air undergoes a similar decomposition, and that we can no more exhibit the antimonial salt in it at the expiration of thirty or forty hours in summer, than we could do if to a like solution we had added albumen or gelatin.

Acetate of lead (sugar of lead).—Some days are sufficient for the acetate of lead, dissolved in water and mixed with animal matters, to be decomposed to such an extent that not an atom of it is found in the liquid; but we can obtain

metallic lead by calcining to a red heat the animal matters and the precipitate of a blackish gray which is formed in the liquid.

Proto-hydrochlorate of tin.—All that has been said of the acetate of lead will apply to this salt.

Sulphate of copper (blue vitriol).—Mixed with animal substances the dissolved deuto-sulphate of copper is decomposed, so that none of it is found, after the lapse of a certain time, in the liquid. This decomposition is not indeed so rapid that we are unable to find a portion of the salt in solution, even after the lapse of some months, if we operate on some drams of the deuto-sulphate. If we are called upon to analyse these matters at the epoch when it would be impossible to detect the coppery salt in the fluids, we must dry the solid substances and carbonise them in order to obtain the metallic copper, whilst another portion of the carbonated matter is to be treated with nitric acid in order to obtain the nitrate of copper.

Verdigris.—By being kept with animal substances under ground verdigris is decomposed and the deutoxide of copper is combined with the fat of the dead bodies so as to produce a kind of soapy matter insoluble in water. In a case of poisoning of this nature, it would be possible to exhibit the presence of the deutoxide of copper several months and even years after burial. All that is required would be to treat the saponaceous mass by the hydrochloric acid, which would dissolve the deutoxide of copper, or to calcinate in order to obtain metallic copper.

Nitrate of silver (lunar caustic).—Nitrate of silver in solution is rapidly decomposed when it is mixed with animal matters; so that we must probably endeavour to obtain this metal from the solid matters, if we are called upon to pronounce on the fact of a poisoning by its salt many days after burial. Experience shows that we can accomplish this purpose even at the expiration of several months.

Hydrochlorate (muriate) *of gold.*—A few days will suffice also to prevent our discovering the hydrochlorate of gold in a solution of the salt exposed to the air and mixed with animal substances; but by having recourse to calcination in the same way as for the nitrate of silver, we obtain metallic gold many months after inhumation.

Acetate of morphia.—It is possible to determine the presence of the acetate of morphia, in a case of juridical disinterment, many months after death. For this purpose we must operate not only on the liquid, but also the solid matters found in the stomach and intestines, because supposing even that the poisoning had been effected by a watery solution of the acetate of morphia, it might have been decomposed and the morphia in part precipitated. Numerous experiments have proved that the acetate of morphia merely dissolved in water and exposed to the air is not long in being *in part* decomposed; the acetic acid of the decomposed portion is destroyed, whilst the morphia is precipitated, if not entirely at least in great part. These results are in accordance with those obtained by MM. Geiger and Dublanc the younger. In a case of juridical exhumation, indeed, if the poisoning had been effected by a watery solution of the acetate of morphia, there would be found less morphia precipitated than we should at first suppose, because a part of that deposited would be redissolved by the ammonia formed during putrefaction. We know in fact that in precipitating the morphia by ammonia in a somewhat dilute solution of the acetate, it is sufficient for us to shake the precipitate for a few minutes with a mixture of ammonia and water to *re-dissolve* it.

In order to obtain the morphia which may exist in the solid matters, we must at first treat these latter with frequent additions of alcohol, then evaporate the alcoholic solution, and expose the product of the evaporation to water with a slight addition of acetic acid; without this latter precaution, it would be difficult to separate the morphia from the fat of the dead body, which is *abundantly* formed while under ground. Should the liquor by chance be coloured, it will be rendered colourless by heating it with *purified* animal charcoal, and filtering it several times through this same body, without having recourse to the sub-ace-

tate of lead and the hydrosulphuric acid, the employment of which appears useless.

In comparing the action of the nitric acid and the test of hydrochlorate of iron on the matters which constitute the subject of these experiments, it is easy to perceive that the nitric acid has constantly reddened them, even though they had been a little coloured previously, whereas the chalybeate salt only *blued* them in general in as far as they had been perfectly discoloured; and again in certain cases it developed a reddish colour though these matters were colourless.

It would be very censurable were we to pronounce *affirmatively*, in a case of juridical exhumation, that there had been poisoning by a preparation of morphia, because we had observed *only* the two colorations, *red* and *blue*, of which we have just spoken. The evidence from these changes is only presumptive. It would not be so if we obtained many months after death, as happened to MM. Orfila and Leseur, a whitish crystallised substance, insoluble in water and sulphuric ether, fusible in a mild heat, becoming *red* by nitric acid, and *bluish* by the salt of iron, and exhibiting in a word all the known characters of morphia: we ought not under such circumstances to hesitate *affirming* that the suspected matter is *morphia*.

Hydrochlorate of brucia.—The presence of brucia and hydrochlorate of brucia may be rendered evident in the digestive canal of the bodies of persons poisoned by these substances, and buried for many months. But here, as in the case of the acetate of morphia, the phenomenon of coloration, developed by the nitric acid and the proto-hydrochlorate of tin, ought not to be considered as evidences of poisoning; and we must, in order to be able to *affirm* positively the fact, separate the brucia and determine their peculiar properties.

Acetate of strychnia.—This salt has been recognised in solution many months after having mixed it with animal matters and water,—the vessel containing it being exposed to the air. In other respects the means of analysis employed do not differ from those commonly put in practice to exhibit the presence of the preparations of strychnia.

Hydrocyanic (Prussic) *acid.*—We learn from the experiments of M. Lassaigne that it is not possible to display, by chemical means, the presence of small quantities of hydrocyanic acid three days after death; the disappearance of the poison depends on the decomposition which it undergoes.

Opium.—There is greater difficulty in detecting morphia in a body disinterred, into the digestive canal of which opium had been introduced, than if we had to deal simply with a salt of morphia. This difficulty is at times insurmountable; hence the authors of this memoir are of opinion, that in no case must we pronounce *affirmatively* of the fact of poisoning by opium, unless we can recognise this substance by its *physical* and *chemical* properties, or obtain morphia which shall exhibit all the characters already indicated. Nor must we even in this latter case conclude in a positive manner, that poisoning has taken place by opium, but rather by opium or morphia, or acetate of morphia.

Cantharides.—A dram of pulverised cantharides has been introduced into a portion of small intestine, with meat and albumen; the intestine was tied at both ends and put into a thin deal box, which was then buried at the depth of two and a half feet. The exhumation of the box took place after nine months and five days, and it was easy to see at this time, that the intestine had been converted into the fat of dead bodies. Scattered through this substance could be seen, even with the naked eye, a multitude of brilliant points of a *superb green*, formed by the powder of the cantharides. By treating the mass with boiling water, the fatty matter was melted, and came to the surface of the liquid in the form of an oily coat, while the shining particles were seen at the bottom of the vessel. A sufficient quantity of these particles could also be collected to enable the experimenters to determine that they possessed all the properties of pulverised cantharides.

MM. Orfila and Leseur conclude their memoir by introducing a reply to the following question. The poisons which have been detected as above in different disinterments had, some will say, been placed in contact with the organs

only after death. Can we then conclude that they would have been found on investigation of the bodies of persons killed by poisoning? The reply is *affirmative*, provided at the time of death there was in the digestive canal a quantity of poisonous substance appreciable by chemical means. Of what moment is it after all whether the action of a poison on our tissues be different during life and after death? Of what avail for us to know that a portion of this poison has been absorbed or rejected with the matters vomited, or passed by stool during the life-time of the individual? The capital point for us to be apprised of is that the quantity of the venomous substance which the medical examiner could discover twenty-four hours after death, can be also ascertained ten, fifteen, or twenty months after interment. Now there cannot from the above detailed results or experiments be any doubt on this head, since these venomous substances cannot act or be acted on in a different manner in the digestive canal of a body buried, from what they were in the stomach and intestines in which they had been enclosed, after their having been mixed with alimentary matters.

VI. SURGERY.

29. *Old Luxations.*—In the seventh volume of the Philadelphia Journal of the Medical and Physical Sciences, professor GIBSON has recorded the case of James Scofield, æt. 50 years, who had his left os humeri dislocated at the shoulder joint early in March 1822. About three weeks after the accident, an attempt at reduction was unsuccessfully made. On the 10th of May, two months after the accident, he consulted Dr GIBSON, and on the 12th, the reduction was effected at the Alms-House Infirmary by extension and counter-extension with bandages and pullies, after endeavouring to break up the adhesions by rotating the arm in different directions, and assisting the above measures by free bleeding from the arm. Half an hour after the operation, some swelling, attributed to inflammation, was noticed; this gradually increased without pain or uneasiness; the patient became pale and pulseless, and died in the evening.

Dissection exhibited a minute orifice in the subclavian vein, and a complete rupture of the axillary artery owing to the firm adhesion of this vessel with the capsule of the joint where it surrounded the neck of the bone. Much blood was effused.

In the third number of the American Journal of the Medical Sciences, Dr GIBSON records another unfortunate accident of the same kind. John Langton, æt. 35, of intemperate habits, applied for assistance nine weeks after a dislocation had occurred at the shoulder joint, and after several forcible attempts at reduction had been made. On the 6th of March, he was admitted in the Alms-House Infirmary, and four days afterwards was submitted to a decided antiphlogistic course; being bled daily to ℥x, kept on barley water, and purged occasionally. The shoulder was rubbed with hot oils and the arm gently moved several times a day, in various directions. On March 15th, reduction was accomplished at the expiration of — hours and — minutes by means of a counter-extending band fixed to a staple, and extending band attached to a pully, assisted by free bleeding and the use of tartrite of antimony to induce nausea. Next day, March 16th, there was a general swelling, but not more than usual on such occasions; it however manifested a distinct pulsation of an aneurismal character. March 17th, the aneurismal symptoms have increased. At 3 P. M. it was decided that the subclavian artery should be secured without delay. This was done by Dr GIBSON.

A detailed account of the case is given; the circulation did not completely return in the limb—the hand and forearm manifested symptoms of gangrene; the patient became prostrated and died.

Dissection.—The subclavian artery was found secured by the ligature near the scalenus; below, this vessel was found ruptured, owing to a firm and close attachment having existed to the os humeri and the articular capsule by dense

ligamentous substance. From the appearances, there was some reason to believe that a small aneurismal tumour had formed, perhaps from previous attempts at reduction, which tumour was ruptured on the 16th of March, giving rise to a diffused aneurism. Considerable quantities of blood were extravasated, but were partially confined by the adhesions.

These cases are certainly unfortunate, especially as no human ingenuity could have previously detected the adhesion of the artery and bone. Similar accidents are exceedingly rare, and hence the best surgeons have unhesitatingly recommended forcible rotation of the bone to tear up its adhesions to the muscles, cellular membranes, ligaments, &c. Dr GIBSON is not able to find any very decided cases similar to his own, in the records of surgery. DESSAULT mentions that in one case, a large tumour, supposed at first to be sanguineous, and afterwards emphysematous, formed under the pectoral muscle immediately after reduction. This disappeared and the patient recovered. PELLETAN alludes to several cases, and mentions that tradition has preserved an account of a case as having occurred at the Hôtel-Dieu, in which a tumour formed after the bone was reduced. It was regarded as emphysematous and punctured—the patient died of hemorrhage. The only authenticated cases, however, collected by Dr G., have been lately (1827) published by M. FLAUBERT, [*Flanbert?*] chief surgeon of the Hôtel Dieu, at Rouen. Vide Medico-Chirurg. Rev. 11th vol. p. 452, and Ephémérid. Médical. de Montpellier, March 1828. In the first case, reduction was effected *eleven days* after the accident by M. LEUDET. The patient died on the 14th day after the operation; signs of ruptured artery, gangrene of the arm and hemorrhage from two openings below the axilla having previously occurred in regular succession. *Dissection* displayed the pectoralis major almost completely torn across the upper portion of the short head of the biceps, and the axillary artery was also ruptured. This vessel adhered to the rib by means of effused lymph. In two other cases tumours formed, followed by irritation, syncope, loss of pulse, &c. from which the patients narrowly escaped. One of these was a luxation of the fore arm backward; the second a luxation at the shoulder joint. Two examples of injuries of the *nerves* caused by forcible measures for reducing dislocated humours are also given by M. FLAUBERT. In one, the axillary plexus seemed to be injured. The patient recovered with a permanent paralysis. In another, a woman, æt. 70 years, had a luxation of the humerus for five weeks; a circumstance at first unknown to M. FLAUBERT. The reduction caused violent pain and induced symptoms of threatened suffocation, followed by pallor, nausea, coldness of the affected side of the body and even a transitory syncope, afterwards by a hemiplegia, which continued till death; this event occurred 18 days after the operation. *The nerves of the arm, adhering to each other, had been torn from the spinal marrow at their origin.*

30. *Sanguineous Tumours of the Female Mammæ.*—In the Ed. Med. and Surg. Journ. for July, Dr JOHN RODMAN has given an account of some collections of blood in the mammæ, the nature of which is obscure, and the event has sometimes been fatal. They appear to originate in the cellular, not in the glandular tissue of the breast; sometimes without any evident cause, occasionally as the consequence of slight mechanical injury. Three stages may be noticed. In the *first*, there is a gradual and moderate enlargement of the mamma, easily to be distinguished from the periodical fulness, which arises from uterine excitement, and also from any kind of inflammatory irritation, although subsequently a kind of inflammatory action of a chronic character may be excited, causing some firmness to be felt in a few cases of the disease. In the *second* stage, a tumour may be distinctly observed, as a small conical or oval body, not very moveable, having "a circumscribed pulpiness, and a kind of soft doughy elastic firmness, with a peculiar smoothness of circular and compressible exterior." Its uniformly ovoid character, its degree of mobility, its uniformity, and a notable obtuseness of feeling, with its situation in the cellular tissue, are diagnostic circumstances. This stage may be short, or, as in most cases, it may last for months or years. The

third stage is characterised by a great enlargement of the tumour from the accumulation of sanguineous fluids; giving rise to a painful sense of distention, enlarged cutaneous veins, thinning of the integuments, &c. The cyst in this stage is delicate, with little vascularity, and with slight attachment to the surrounding parts. The contents vary; consisting sometimes of a dark coloured grumous, thick, and tenacious bloody matter, often merely of a sanguineous fluid, and sometimes, no red particles are observed, a serous fluid only existing in the sac. Cases are given from authors and from Dr RODMAN's own practice, from the results of which he would recommend that during the *treatment*, attention should be paid to the stages of the disease. Pressure by bandages proves highly useful in the second and third stages, but less so in the first stage. The repeated evacuation of the contents, by punctures, not by incisions, when the tumour is bulky, in order to prepare for the modes of treatment, is advantageous in the third stage alone. Punctures, even very frequently made, are useful in relieving the distention, in preventing the bad consequences which would hence result, and in facilitating the contraction of the cyst and the consequent diminution of the diseased growth. The cure may often be hastened by injecting vinous or other fluids into the cyst as in the treatment of hydrocele. Extirpation in the second stage has been useful; in the advanced stage it is almost superfluous, as the gland disappears spontaneously. The general health is usually disturbed and requires appropriate treatment, as all local affections are influenced by constitutional disturbance.

31. *Tetanus.*—Mr EDWARD LEAH, assistant surgeon of H. M. S. Albion, records in the Ed. Journ. for July, two cases of tetanus on board the Albion after the action of *Navarino*. In one, tetanus succeeded a gunshot wound of the fleshy part of the arm on the seventh day, and proved fatal with great rapidity, although the system was brought effectually under the influence of mercury—and large doses of laudanum were freely given. In the second, the spasmodic disease began at a later period, after an amputation of the shoulder joint, rendered necessary by a severe cannon shot wound. The lower jaw was nearly closed for several days and spasmodic rigidity of the abdominal muscles occurred. The patient recovered under the moderate use of opium, and a careful attention to diet, regimen, and the general state of his health. His bowels were opened by mercurial and colocynth pills, occasionally repeated.—Laudanum in doses of 80 or 90 drops was prescribed at night, and the linimentum ammoniæ wi'h laudanum used to the jaws. Spoon meat was freely allowed. His mouth was, at one time, a little affected with the calomel. Port wine was allowed, the wound granulated and cicatrized, and the patient gained rapidly both flesh and strength.

Another case of recovery from traumatic tetanus occurred after the same battle, under the care of Mr SANKEY, surgeon of the brig Musquito. The arm was shot away close to the body, and the patient soon after had his jaw fixed; the muscles of the neck and abdomen quite rigid, and the spasms occasionally severe. Moderate doses of opium and camphor with the sulphate of quinia and nutritious diet were relied on with success.

32. *Amputation of the Lower Jaw.*—In the ninth number of this journal will be a notice of Mr CUSACK's cases of disarticulation of the lower jaw. Mr LISTON of Edinburgh has since reported a case of osteo-sarcoma of the right side, for which he extirpated that portion of the bone between the canine tooth and the angle. The patient apparently recovered in a few weeks, but, soon afterwards, the disease re-appeared in the upper remaining portion of the bone, and Mr LISTON successfully removed this fragment at the joint. The hemorrhage was arrested by a single ligature on the common trunk of the temporal and maxillary artery. The patient recovered with loss of motion, not of sensation, of the right side of the face.—*Ed. Med. & Surg. Journ. July*, 1828.

33. *Chronic Inflammation of the Joints.*—The July number of the Ed. Med.

and Surg. Journ. contains a review of a small octavo volume lately published by Mr JOHN SCOTT of London on the treatment of Chronic Inflammation, especially of the joints. Perhaps there is nothing strictly new in the principles and practice here recommended, especially to those American surgeons who are acquainted with the uniform practice pursued for many years with great success by Dr PHYSICK—*of keeping the diseased joint perfectly at rest* by means of suitable splints, and in pursuing a course of purging as the best method of causing revulsion. Mr SCOTT's method is somewhat different; his principles very similar.

Inflammation, involving the whole structure of a joint, has its origin usually in one of these tissues,—in the synovial capsule, in the cartilages, or in the bones, each of which may be generally detected. Eventually all become affected; lymphatic and purulent effusions occur, followed by abscesses, caries, ulcerations, and fistulæ, with great constitutional irritation. During the first stage, when there is much heat and pain in the joint, in addition to *absolute rest*, local bleeding, by cups or leeches, is very important, to be repeated on the continuance or return of these symptoms, provided, however, the constitutional powers are not greatly impaired; for, in such cases, Mr S. thinks local bleeding, instead of abating, aggravates the disease. The common bread and water poultice is the best application after the bleeding; cold lotions are not recommended, as they tend to repress rather than encourage the cutaneous circulation. When the above measures have been pursued sufficiently far, Mr S. would not resort to the usual modes of revulsion by blisters, issues, &c. &c.—not that they are invariably useless, but they are always uncertain in their operation, often aggravate the patient's sufferings, and in many instances directly increase the violence of the disease by their irritating character,—the inflammation excited by them being propagated to the articular textures. Mr S. therefore substitutes the following method. "In the first place, the surface of the joint, suppose the knee, is to be carefully cleaned by a sponge, soft brown soap, and warm water, and then thoroughly dried; next, this surface is to be rubbed by a sponge, soaked in camphorated spirit of wine, and this is continued a minute or two, until it begins to feel warm, smarts somewhat, and looks red. It is now covered with a soft cerate made with equal parts of the ceratum saponis, and unguentum hydrargyri fortius cum camphorâ. This is thickly spread on large square pieces of lint, and applied entirely round the joint, extending from at least six inches above and below the point at which the condyles of the femur are opposed to the head of the tibia; over this, to the same extent, the limb is to be uniformly supported by strips of calico, spread with the emplastrum plumbi of the London Pharmacopœia. These strips are about one inch and a half broad and vary in length; some are fifteen inches, others a foot, others half these two lengths, and the shorter or longer are selected, according to the size of the part round which they are to be applied. This adhesive bandage ought to be so applied as to preclude the motion of the joint, prevent the feeble coats of the blood vessels from being distended by the gravitation of their contents in the erect posture, and thereby promote their contraction. Over this adhesive bandage, thus applied, comes an additional covering of *emplastrum saponis* spread on thick leather, and cut in four broad pieces, one for the front, the other for the back, and the two others for the sides of the joint. Lastly, the whole is secured by means of a calico bandage, which is put on very gently, and rather for the purpose of securing the plaster, and giving greater thickness and security to the whole than for the purpose of compressing the joint. This is an important point, as otherwise an application, which almost invariably affords security and ease, may occasion pain with all its attendant mischief."

The objects of this treatment appear to be, first, to induce a gentle but general excited state of the cutaneous and superficial vessels of the joint; secondly, to prevent all motion; and thirdly, by this and by the mechanical support to abate the chronic congested state of the vessels. Sometimes the first object is not sufficiently obtained by the camphorated spirits, and then the tartar emetic ointment may be useful; and when the second indication is not fulfilled by the plasters, pasteboard splints are recommended. These dressings need not be removed more than once a week, unless some peculiar circumstances require it, such as

the recurrence of heat or pain demanding local bleeding, or the discharge of matter, or the cessation of the superficial irritation.

This treatment is suited to every stage of articular disease, excepting that of acute inflammation, and has succeeded, according to Mr S., more frequently than any other plan, and that too in some of the worst forms and stages of the complaint—where the bones, cartilages and membranes are involved, sinuses formed, and severe constitutional irritation excited; the latter symptoms of course receiving suitable attention. Cases of this character are given.

Mr S. objects to the practice of puncturing the joint when distended with matter as advised by Mr BRODIE. If pressure, as above recommended, does not answer for the dispersion of the pus, its escape by ulceration is a safer mode of termination. He objects also to stimulating injections into the fistulæ; if the bone is merely carious, not necrosed, it is unnecessary to cause an exfoliation which would not otherwise have occurred. If really dead, the removal by art would induce a more extensive exfoliation, than if left to the absorbents.

We hope these details will encourage American surgeons in the scientific treatment of these tedious but still remedial diseases.

34. *High Operation.*—At the sitting of the Section of Surgery on the 14th of February, and on the 13th and 27th of March, 1828, a very animated discussion occurred as to the advantages and dangers of the high operation for calculus. MM. SOUBERBIELLE, ROUX, LARREY, RIBES, DUBOIS, LISFRANC, and AMUSSAT were the principal speakers—men well acquainted with the theory and practice of surgery. The subject was introduced by a letter from M. S., who, our readers will remember, has of late years revived this operation, stating that his octogenarian patient (vide the last No. of this Journal, p. 191.) had recovered without any accident, and that this individual was the seventh cured consecutively by the method of FRERE COME, without any perineal incision, or the introduction of an elastic catheter through the wound, or of strips of linen, or the employment of sutures in the bladder, as recently again recommended. This method is useless, and even aggravates the dangers. To prevent the urine passing by the wound, M. S. resorts to an apparatus which he terms *a compound syphon*, consisting of two or three elastic catheters attached one to the other by their extremities, and fitted to the catheter introduced per urethram into the bladder, after having filled them with water. In this way, the flow of urine is fully established even when it is thick or loaded with mucus, and all danger of urinary infiltration at the wound is prevented.

The discussion turned upon the points—whether the urine frequently escaped at the wound; and whether, under such circumstances, there was danger of urinary infiltration giving rise to inflammation, abscess, &c. M. AMUSSAT, a graduate of 1826, after a few operations, delivered his opinions with great confidence, that, unless his method was adopted, it was physiologically impossible that the urine should not pass by the wound, that it was a want of good faith to deny this occurrence, that the bladder, as divided in the operation above the pubis, is no longer a complete pouch, and that the intestines, pressing upon this viscus, prevent the accumulation of urine in its cavity, and force this fluid through the wound. On the contrary, MM. RIBES, GENDRIN, and SOUBERBIELLE assert that in eight patients operated on by the latter gentleman, the urine escaped from the wound only in one case. They had each examined the patients. LARREY, after much experience in forty years constant and varied practice, affirms that there is very little danger in the escape of urine from the bladder, as is constantly observed in wounds of this viscus: that this circumstance has absolutely no influence on the success of the high operation, and that instruments introduced into the wound are perfectly useless and often dangerous.

Some further discussion ensued on the 13th of March, when M. ROUX reported the case of an ecclesiastic, aged seventy-two years, on whom he performed the high operation. He distended the bladder by injection, according to the method of ROUSSET, and was thus enabled to penetrate it without difficulty above the pubis. Two calculi were extracted and a catheter left in the urethra.

The urine flowed entirely through the wound, and until the fourth day no bad symptom ensued. Then the scrotum swelled; and a tumour formed at the root of the penis; these swellings suppurated rapidly: the patient became exhausted and died on the eighth day. *Post mortem*—no infiltration of urine was seen in the pelvis, but it had occurred in front at the inferior angle of the wound. The symphisis of the pubis was denuded, and a stone was found encysted at the inferior part of the right ureter.

The discussion turned on the necessity of introducing a catheter into the wound to carry off the urine. This M. AMUSSAT insisted upon; and DUBOIS observed, if this practice were proper, a very large catheter would be requisite so as to fill completely the opening in the bladder. This was denied by M. AMUSSAT, who maintained that the wound contracted exceedingly, so that no urine would escape at the side of the instrument. M. LARREY remarked that a catheter in the urethra would not remove the urine as rapidly as it arrives in the bladder, as its point would be too elevated. The instrument, he would say, should be as large as possible, and in some cases it might be proper to dilate the urethra previously to the operation, that it may admit a large catheter. The presence of the catheter causes no inconvenience, and his practice taught him that it was often sufficient to remove the urine as rapidly as it arrives in the bladder; and even were this not practicable, *the escape of the urine by the wound was altogether without injury*. M. AMUSSAT answered that it was "impossible" that the urine could escape through a catheter in the urethra after the high operation; because, when the bladder has contracted, the wound is in a more depending position than the mouth of the urethra: the solution of continuity is then very near the prostate, and the urine naturally flows towards the lowest point and passes through the wound or the canula which occupies the opening. The sitting of March 27 added no material facts to those above stated.

35. *Abscess within the Cranium cured by Trephining.*—M. ROUX reports (Jour. Général. de Méd. Avril, 1828) the case of a boy, fifteen years old, who, for four years, had a fistulous opening in the left parietal region, caused by an abscess from a blow on the head. Whenever the pus accumulated, comatose symptoms supervened. M. ROUX applied the crown of a trephine to the opening in the cranium, to facilitate the removal of necrosed bone, which he imagined was the cause of the mischief. No dead bone was discovered; the fistula penetrated the dura mater; this membrane was incised and an abscess thus exposed pressing on the brain. The pus being evacuated, a considerable depression was observed on the left middle lobe of the brain. The depression gradually disappeared; the orifice from the trephine filled up, and the patient recovered. This termination of the operation was altogether unexpected.

Several similar cases are upon record; and we feel not a little surprised that surgeons of the present day should any longer hesitate in applying the common principles for the treatment of abscesses and fistulæ to such complaints when involving the contents of the cranium. Why an abscess of the brain, increasing inflammation, and causing compression, convulsions, paralysis, coma, and death, should not be penetrated, and its contents discharged as in similar cases in other tissues or organs, and why the patient's chance for recovery should not be greater after the puncture has been made than when left to unassisted nature, has not been explained. We believe that the results of experience and the deductions of reason are decidedly in favour of a judicious scientific interference on the part of the surgeon.

Since writing the above, we have read in the Journ. Univer. for April, a case of abscess in the brain, treated by the justly celebrated DUPUYTREN, which we think confirms the observations already made. A man, aged forty-five years, entered the Hotel-Dieu on the 8th of June, having been delirious for fifteen days, in consequence of a blow on the head. A large wound, exposing the bone, existed in the forehead. Sensibility, power of motion, respiration, digestion, and the organs of sense were unimpaired. The pulse was natural, as also his excretions. There was no loss of memory; his delirium was characterised by an in-

exhaustible loquacity and incoherency of ideas. M. DUPUYTREN suspected an effusion of blood, "the absence of every febrile symptom did not allow of the suspicion of an abscess" (!!). On the 17th the trephine was applied to determine whether the bone was necrosed, and to ascertain whether there was effusion between the bone and dura mater. Finding the bone becoming red and humid, he was satisfied that no effusion existed, and went no further in the operation. On the 20th, restlesness, agitation; delirium as before. At 4 o'clock, P. M. an enema with ten drops of laudanum was administered. At six, the patient was in a profound stupor, from which he could not be roused. On the 21st, in the morning, again sensible; some symptoms of hemiplegia on the left side. On the 22d, hemiplegia more marked—urine retained; catheterism. On the 23d, symptoms aggravated. The catheter gave issue to a large quantity of urine; was trephined; no effusion on the surface of the membranes. On the 24th, patient died. On the 26th, *dissection*. A bistoury being plunged into the right hemisphere of the brain, a large quantity of sero-purulent fluid flowed out. All the cephalic mass on this side, as far as the corpus callosum, was disorganised, and presented a granulated surface of a whitish grey colour, resembling the tuberculous cavities in the lungs. The surface of the brain was ulcerated and covered with pus at the superior and middle part. There was an effusion of blood of some size in the middle of the lateral ventricle on the right side. There was no fracture and no alteration of the membranes.

Comparing this with the case of M. ROUX and many others of an analogous character, we must infer that the patient's chance of recovery would have been greatly improved if the abscess of the brain had been early opened. Vide Dr DUDLEY'S cases, page 195 of the present volume of this journal.

36. *Staphyloraphy*.—"During eight years, since M. ROUX has resorted to this operation, forty individuals have submitted to it. In nineteen, the division did not extend beyond the soft palate; six only have not been cured. In the remaining twenty-one, the congenital division extended to the arch of the palate; of these, nine have been cured by the operation and the remainder have been placed in a condition to wear an artificial palate (un obturateur)."—*Journ. Général. Avril*, 1828.

37. *Cold Abscesses*.—It is well known to the profession that very dangerous symptoms result in most instances, when large chronic or cold abscesses, as they are termed by the French, are freely opened. Much discussion has existed as to the cause of the inordinate inflammation and the severe general affection which thus supervened. No doubt, much is owing to the decomposition of the purulent fluid remaining in the abscess, which thus becomes a source of irritation. M. LISFRANC (says the Archiv. Général for June) has acted on this idea in treating a cold abscess of the upper part of the arm in which the pus was vitiated. He emptied the cavity completely and prevented the secondary inflammation by applying thirty leeches near the part. At the end of three days, the discharge had nearly ceased, and the patient was completely well in twelve days.

38. *Amputation at the Hip Joint*.—M. DELPECH, of Montpellier, reported to the Royal Academy of Medicine at Paris at its sitting on the 26th of April, two cases of amputation at the joint of the hip. In one case there was necrosis of the thigh bone; the operation was performed; the cicatrix was completed in thirty days, and the patient recovered. In the second case, there was an unconsolidated fracture of the femur in which the seton had failed. In twenty days, the wound from the operation of amputation at the joint was almost well; abdominal disease supervened, and the patient died six months after the operation. In both cases, the femoral artery was previously secured. M. DELPECH recommends that one flap only be made, and that of integuments on the outside of the limb, so that the cotyloid cavity shall be completely covered. M. LARREY advises two flaps as at least equally advantageous.—*Archiv. Général. for June.*

39. *Cauterisation of the Cornea*.—M. DEMOURS, on behalf of a committee

of the Academy of Medicine, read a report on the 8th of May, on an essay of M. SERRES, physician at Uzés, entitled—"*On the cauterisation of the cornea, for correcting, in a prompt and sure manner, alterations of sight with dilated pupils.*" M. SERRES proposes to treat idiopathic paralysis of the iris, without affection of the retina and optic nerve, by applying nitrate of silver to the cornea near its junction with the sclerotica. Four cases are given by him, and the committee of the Academy, having employed this remedy in three cases, bear testimony to its efficacy. The caustic should be applied for one second, and it is useful that some lachrymation should be excited and also a slight injection of the vessels of the conjunctiva. The light cloud, which appears on the cornea, vanishes in a few days.—*Archiv. Général. June.*

40. *Aneurism of the Brachial Artery cured by Pressure.*—M. TETU, a regimental surgeon, details the particulars of a case of false aneurism at the bend of the arm, cured by pressure, which method he prefers to the ligature now so universally recommended. His patient in twenty-one days was cured—the tumour having disappeared, and the artery being obliterated for four inches. This is the second case in which he succeeded; the first patient recovered in sixteen days. His mode of compression is by a bandage methodically applied from the hand, by a graduated compress over the tumour and in course of the artery, and covered by a splint two inches wide and fifteen in length. Another splint, four inches wide and also fifteen long, was applied on the opposite side of the arm. Both were secured by a bandage, so as partially to interrupt the blood in the injured artery. In a few days, the pressure was increased, so as to arrest the passage of blood in the vessel; the circulation, however, continued in the anastomosing vessels, owing to the arrangement of the splints by which they were protected from compression.—*Journ. Univer. May.*

41. *Puncturing the Dura Mater.*—There are but few cases on record in which patients have survived when the dura mater has been punctured by a surgeon for the evacuation of effused blood; and most surgeons unite in opinion as to the danger of the operation. [Vide first volume of this Journal, p. 182, for a notice of the dangers, &c.] At the Medico-Chirurgical Society, Mr BRODIE related the following case: A woman fell into a cellar, and pitched on her head. She lay senseless and apparently in profound coma or apoplexy. Mr OGLE examined the head after it was shaved with great care, but could discover no mark of fracture or other injury. When pressure, however, was made on a particular portion of one of the parietal bones, the patient appeared to evince some signs of sensibility. After employing the usual means, and waiting a reasonable time without symptoms of returning sensibility, Mr OGLE determined to apply the trephine over the part above mentioned. A portion of bone being removed, the dura mater of a blue colour bulged out, and was soon on a level with the external surface of the surrounding bone. The sense of touch convinced Mr OGLE that there was blood effused under the dura mater. He therefore punctured this membrane, and blood immediately spouted forth. The patient almost immediately recovered sensibility, and eagerly demanded "what they were about." She recovered.—*Medico-Chirurg. Rev. July*, 1828.

42. *Concussion.*—At the Medico-Chirurgical Society, two cases of concussion of the brain were detailed. In one the patient remained insensible for 17 days; in the other the insensibility continued *three weeks;* both patients eventually awoke, and recovered without a bad symptom. No treatment was resorted to.—*Medico-Chirurg. Rev. July.*

43. *Dissecting Wounds.*—The well known terrible consequences often resulting from these accidents—trivial in appearance—have been attributed by some to the absorption of a poison, and by others to a peculiar predisposition

of the patient's system. Mr A. Cooper appears, *at present*, to be inclined to the former opinion, from what we learn from the Med. Chir. Review for July last, although he has, if we mistake not, openly advocated the idea that a predisposition was requisite. He says, "It would seem under certain circumstances, a poison is produced sufficiently strong to excite inflammation, even when there is no wound."—"Mr Cook, surgeon, sent to me whilst labouring under the highest irritative fever, in consequence of having opened the body of a person who had died of puerperal fever. When I examined him, I found the extremities of his fingers of both hands inflamed, as if they had been dipped in scalding water, and the absorbents of his arms red, hard, and knotted to the axilla; yet he had not any wound or abrasion of any kind upon his hands, and it would therefore seem that the fluid produced in the abdomen of this woman, in which his fingers had been frequently immersed, was of a highly stimulating nature."—*Vol. III. Lect. Surg.*

44. *Treatment of Burns with Raw Cotton.*—The practice of treating burns with raw cotton, which originated in the United States, has been successfully imitated by Dr Anderson, who gives the results of his experience with it in a paper published in the Glasgow Medical Journal for May 1828.

Dr Anderson states, that the most striking effect of the cotton is to allay pain and irritation, rapidly and altogether, in all burns, whether characterised by simple vesication, or deep disorganization and suppuration. Some of his patients, who had been previously treated by the old method, bore marked testimony to the advantages of the cotton in relieving the pain; and he remarks that even in hopeless cases, in consequence of the extent of the injury, the application of the cotton is speedily followed by great relief, and even a total subsidence of suffering. The consequence of this is, that the heat of the body diminishes, the anxiety ceases, sleep is restored, and the appetite returns.

By thus checking inflammatory action, the cotton in superficial burns always accelerates the cure, often completing it, without leaving any material scar. Dr Anderson conceives, that the cotton forms, with the discharges, a substitute for the epidermis, and thus affords a protection, which enables the process for the formation of the new cuticle to take place, undisturbed by external irritations. In the case of a collier, who was extensively burnt, the cotton was allowed to remain undisturbed for fourteen days; and when the mould of matted cotton was removed, it was found that many inches of the circumference of the sores were healed, their centres presenting, not granulations, but the denuded true skin, with its papillæ inflamed and enlarged, so as to present a granular appearance. In this case, the patient got rid of the marks of small pox.

Dr Anderson made a comparative experiment on a girl, who had both legs similarly and severely burnt. The leg dressed with cotton gave her little uneasiness, and when the cotton was removed at the end of the third week, a new and sound skin presented itself. The other leg which was dressed with the linimentum calcis, continued long inflamed and acutely painful, and several troublesome ulcers resulted, which were not entirely healed until three months had elapsed.

When a burn is severe and deep, it is known frequently to kill the true skin and parts underneath, and the consequent slough is usually a long time in being separated. Frequently the patient sinks under the violence of the inflammation or the profuseness of the discharge, or if he escapes, the cicatrix is usually contracted. Dr Anderson conceives that these evils are greatly mitigated by the use of cotton, and sustains his assertion by the history of a very severe case of burn in which cotton was the principal remedy, and which ended in recovery.

Dr Anderson directs, that the cotton should be finely carded in narrow fleeces, thin enough to be translucent. If vesications exist, the serum is to be let out, and the parts washed with tepid water. If the skin is deeply scorched, a spirituous or turpentine lotion is better. The cotton is then applied in successive layers, until the injured parts are completely protected from the effects of motion and pressure; and a bandage may sometimes be necessary to retain it.

When the discharge has soaked through the cotton, or become insupportably offensive, the layers should be removed as far as can be done without exposing the injured parts, and fresh layers substituted. The first dressings should be allowed to remain undisturbed as long as possible.—*Edinb. Med. and Surg. Journal for July, from the Glasgow Med. Journal.*

VII. MIDWIFERY.

45. *Cæsarian Section.*—Mr JOHN CRICHTON performed this operation, in April 1826, on a female aged 22 years, whose pelvis was greatly deformed in consequence of a severe injury sustained at a threshing mill some years previously. Labour had continued for several days, and fatal symptoms had appeared; there was reason to believe the child was living. The operation was readily performed, and the child removed in a state of asphyxia, but was soon revived. The mother however died in a few hours. A portion of the scalp of the child spacelated from the long continued pressure on the pubes, nevertheless he throve well, and is now (May 1828) a stout healthy boy.—*Ed. Med. and Surg. Jour. July.*

46. *Retention of the Placenta.*—In RUST's Magazine is related a case by Dr HOFFMANN, in which the practice proposed by M. MOJON, of injecting the vessels of the placenta with fluids to promote its expulsion in case of retention (vide the second volume of this Journal, p. 438) was successfully imitated. In this instance, after delivery of the child, the placenta remained adherent, the os uteri contracted, the abdomen and uterus enlarged, while the paleness, faintness, and the loss of pulse indicated internal hemorrhage. The injection of some warm water and spirits was in one minute followed by the contraction of the uterus, and subsequently by the expulsion of the placenta. The patient recovered rapidly.—*Jour. des Progrès, Vol. IX. p.* 233.

This practice is exciting attention. A long essay, including the detail of cases, has been presented to the Medical Society of Paris, at its meeting in January last, and was reported on very favourably by MM. DUCHATEAU, CHAILLY and GENDRIN. The essay and report have been published in full in the April No. of the Jour. Gén. de Méd. The reporters close their labours by observing, "that the memoir of M. LEGRAS is well prepared. The author has endeavoured to restrict himself to actual experience, and he appears to us to have demonstrated by facts the safety in all cases of injections into the vessels of the cord after the birth of the infant, their efficiency in causing a separation of the placenta, in arresting hemorrhage from a partial detachment of this body, and finally in stimulating the uterus in cases of inertia of this organ. The views we have taken, and the experiments we have reported, conduct also to this important practical fact, that the effect of the injections can be regulated by graduating the temperature and quantity of the fluid injected, by rendering it more or less styptic, and by augmenting, at pleasure, the impulse by which it is thrown into the vessels. If a few ounces (say ℥vi.) of fresh water be adequate for the separation of the placenta in ordinary cases, it would be imprudent to trust to this quantity in dangerous hemorrhages from partial detachment of the placenta with absolute inertia of the uterus. In such cases we should rapidly fill and distend the placental vessels, so that the cold and styptic fluid should be driven even to the uterine surface, by thus forcing it to exude (*suinter*) from the placenta."

47. *Abortion.*—In the 9th volume of the Journal des Progrès, p. 237, is related a case of abortion, at the end of the third month of pregnancy, in which the placenta, with one inch of the umbilical cord attached to it, was expelled twelve days before the delivery of the fœtus. The placenta had the size of one belonging to a fœtus of between two and three months; it was perfectly healthy,

had no unpleasant odour, and was perfect in its form. Its expulsion was preceded and accompanied by much hemorrhage and pain, which soon after ceased, so that the patient appeared quite well, resuming her strength, appetite, &c. At the expiration of twelve days, a bloody fœtid discharge came on per vaginam, with pain, chills, &c.; followed after some hours by the delivery of a fœtus, two inches in length, and with a fragment of a cord eighteen inches long, in a state of putrefaction. The age of the fœtus, probably not more than seven or eight weeks, did not correspond with that of the placenta; and hence there is reason to believe that at the end of the eighth week, a rupture of the cord took place, and the consequent death of the child, while the placenta continued to grow until expelled during the third month. Why the placenta should have been thrown off before the fœtus, a fact of very unusual occurrence, and how the infant was retained, must remain a subject of speculation.

48. *Hysterotomy.*—M. KLUGE performed this operation at Berlin on the 24th of August, 1827. The patient was twenty-three years of age, and greatly deformed from disease of the spine, pelvis, &c. Labour supervened at the full period of pregnancy and delivery being impracticable, the Cæsarian operation was performed. The child survived. The mother, a few hours after the operation, was affected with spasms of the muscles of the neck and thorax; with rapid and hard pulse. On the 25th, the nervous symptoms, the hard and frequent pulse increased; vomiting and convulsions supervened with febrile symptoms, &c. She died on the 28th. "*On opening the body no traces of inflammation or of gangrene could be discovered in the whole extent of the intestinal canal, in the peritoneum, in the uterus, nor in any other organ.*" The edges of the external wound did not adhere, neither did those of the incision in the uterus. The lungs, the structure of which was healthy, contained a very small quantity of blood.—*Journ. des Progrès, Vol. IX. p.* 264.

49. *Extirpation of the Uterus by Ligature.*—This operation was lately performed in London on a lady who two years previously suffered an inversion of the uterus from a forcible removal of the placenta, and for a considerable period was sinking from occasional hemorrhage. A ligature was applied and occasionally tightened. At the end of a fortnight, the tumour, formed of the uterus, came away; and at the end of another week (the time of the report) there were no bad symptoms; the patient however continuing weak and pale.—*London Med. and Surg. Journ. for July.*

VIII. CHEMISTRY AND PHARMACY.

50. *Action of Muriatic Acid on Albumen.*—In the "Journal de Chimie Médicale" for July, 1828, M. BONASTRE gives a note on the effect of this acid on different varieties of albumen. He confirms the previous remark of M. CAVENTOU, that the addition of pure muriatic acid to albumen gives rise to a blue colour, although several of his colleagues, on repeating the experiment, did not succeed in producing the phenomenon.

M. BONASTRE has succeeded in producing a blue colour with the following specimens of albumen; namely, the white of eggs, the crystalline lens, and the vegetable albumen existing in the seeds of the *dolichos urens*, the *misosa scandens*, and *phaseolus vulgaris*, or kidney bean.

If any sure and easy method for applying this test could be devised, it would be valuable as a means of detecting animal and vegetable albumen in solutions.

51. *Analysis of Cancerous Matter.*—In the Journal de Chimie Médicale for July, 1828, M. COLLARD DE MARTIGNY gives an analysis of this matter, from which we furnish the following abstract:—

Physical Properties.—A white tissue of a nauseous smell, and lobulated appearance, resisting the scalpel but slightly, and heavier than water. When dried, it was yellowish brown, of a faint odour, slightly brittle, presenting a filamentous and whitish fracture, and semi-transparent.

Chemical Properties.—Half an ounce of the matter, exposed to the air, after having been allowed to remain in water for several hours, became conical, soft, and almost pultaceous. Its smell was strong and nauseous, but of a different character from that of the fresh matter, being less virulent (vireuse). The colour was various, being of a bronze brown on the sides, and yellowish blue at the summit.

Treated with cold water, a slight portion of albumen was dissolved, which precipitated by heat in the form of frothy flocks.

On distillation, it gave no volatile principle. Ether and alcohol, in the cold, had no sensible effect on it.

On being boiled in distilled water, this menstruum dissolved a considerable quantity of gelatin, which was white and perfectly transparent. The insoluble residue was grayish-white, devoid of any fibrous appearance, and soft, and presented all the characters of albumen.

Boiled with alcohol, the matter assumed a beautiful dull white colour, became very elastic and difficult to tear, smooth externally, and fibrous and filamentous internally. The alcohol acquired a greenish colour. Upon being filtered and allowed to cool, it became whitish and milky, and let fall a pulverulent precipitate, which disappeared upon re-heating the alcohol. When evaporated, it assumed a denser and deeper emerald-green colour, and when dryness was attained, a substance was left of a deep yellow colour, and agreeable smell, possessing a medium consistence, and which stained blotting paper, and burned like a fatty body. This matter, which had a decided acidity, and was insoluble in caustic potassa, is considered by M. COLLARD to consist of cerebral fatty matter.

M. COLLARD sums up the results of his analysis as follows:—

Albumen	0.206
Gelatin	0.021
Fatty matter	0.020
Phosphorus and salts	Trace
Water.	1.700.

52. *Coniin, or Active Principle of Cicuta.*—M. BRANDES gives the following process for obtaining this principle:—Digest the leaves and stem of the fresh plant, well bruised, for several days, in alcohol. Filter the solution, and evaporate to dryness. Treat the alcoholic extract with water, and add to the aqueous solution obtained, either magnesia, alumina, or the oxide of lead. Evaporate this solution to dryness, and treat the dry residue with a mixture of alcohol and ether. This menstruum takes up the coniin, which, by a new evaporation to dryness, is left in a pure state.

This principle, according to M. GISEKE, possesses the following properties, by which it may be distinguished:—1st. In contact with tincture of iodine, its solution gives rise to a reddish precipitate. 2d. Tincture of galls renders its solution brown, but causes no precipitate. 3d. It precipitates solutions of the sulphate of mercury and muriate of zinc, of a dirty yellow colour. 4th. It occasions a slight turbidness in solutions of the carbonates of potassa and soda. 5th. It communicates a brown colour to the muriate of platinum. 6th. With the nitrates of silver and baryta, the acetates of baryta and lead, the muriate of lime, and lime-water, it gives rise to grayish-white precipitates.

Half a grain of coniin is sufficient to kill a rabbit. The symptoms induced by it are analogous to those produced by strychnia. After death, the vessels of the head, the right auricle of the heart, the superior vena cava, and the jugular, are very much gorged with blood, while the abdominal vessels appear to be completely empty.—*Archives Générales, June* 1828—*from Arch. Brandes, Vol. XX.* 1827.

53. *Red Ferro-cyanide of Potassium.*—This substance was discovered by GMELIN, and has been proposed by BERZELIUS as a reagent in the examination of mineral waters, as it detects the presence of proto-salts of iron by a green or blue precipitate, while it has no action on the per-salts of the same metal. It is formed by passing a current of chlorine through a solution of the ferro-prussiate of potassa, until this salt no longer precipitates the per-salts of iron, and then filtering the solution, and allowing it to undergo a slow evaporation. This cyanide is in the form of reddish-yellow needles, exhibiting the metallic lustre. According to BERZELIUS, it contains one and a half times as much cyanogen as the ordinary ferro-cyanide of potassium (ferro-prussiate of potassa). M. GIRARDIN has ascertained that it dissolves in twice its weight of cold water, and in its own weight of boiling water. It is not soluble in absolute alcohol. It greens the sirup of violets. As a reagent for iron, it is more sensible than the ferrocyanate of potassa, since it will detect this metal, when present, in solutions four times more dilute than those in which the latter salt can detect it.—*Archives Générales, June* 1828.

54. *Dangers from the different Strengths of Pharmaceutical Preparations of the same Medicinal Agent.*—In the Journal Général de Medicine for June 1828, the editor, Dr GENDRIN, gives an account of the shocking occurrence of the death of seven patients at the hospital of the Bicêtre at Paris, in consequence of their taking a stronger preparation of the hydrocyanic acid than was contemplated by the physician.

It appears that the physician of this hospital, Dr FERRUS, having derived advantage from the administration of hydrocyanic acid in epilepsy, was desirous of giving it a trial at the hospital. He had been in the habit of prescribing it in the form of sirup, as prepared by M. PELLETIER, after the directions given by MAGENDIE in his formulary. The dose, with which Dr FERRUS was in the habit of beginning with this preparation, was half an ounce, as laid down by MAGENDIE.

Dr FERRUS, accordingly, chose as the subjects of his experiments fourteen epileptic patients of the Bicêtre hospital, from the age of fifteen to thirty years. For each of these, he prescribed the sirup of hydrocyanic acid, which, not being in the shop of the hospital, was ordered to be procured from the magazine of the central pharmacy of the hospitals. The medicine having arrived, Dr FERRUS expressed his intention to begin with an ounce of the medicine; as he was satisfied from experience, that this dose was devoid of danger. The hospital apothecary, however, having remarked, that it was not customary in the practice of the house to exceed a drachm of the sirup in twenty-four hours, and that even this dose of the medicine, as prepared at the central pharmacy, was sometimes too powerful, Dr FERRUS confined himself to the prescription of half an ounce of the sirup, to be taken at once. Accordingly, on the 17th of May, half an ounce of the sirup of hydrocyanic acid, as obtained from the central pharmacy, was commenced to be given to each of the epileptics. The nurse had already administered it to seven of the patients, when he observed that one of them was rendered ill. This induced him to examine the others, when he found the first already dead, the second just expiring, and the third exhibiting all the symptoms of being poisoned. At the end of fifteen or twenty minutes, the seven patients were all dead!

It may be easily imagined that so shocking an occurrence in a large establishment like the Bicêtre produced a great sensation in Paris. Public clamour spread the facts, which very soon came to the knowledge of the judicial authorities; and an inquest was immediately instituted, by the king's attorney, to investigate the circumstances. It was ascertained that the nurse had made no mistake, nor had any been committed in the preparation of the article, at the laboratory of the central pharmacy, it having been prepared precisely according to the formula of the French Codex.

The shocking catastrophe plainly arose from the circumstance, that an active medicine was prepared under the same name, of such opposite strengths, as to

constitute, in the same dose, a medicinal article and a most deadly poison. Dr FERRUS had been in the habit of prescribing MAGENDIE's sirup in his city practice, and by a fatal mistake the sirup of the Codex was given at the hospital.

Dr GENDRIN states that the sirup of the French Codex consists of simple sirup, nine parts, and medicinal hydrocyanic acid, one part; while MAGENDIE's formula directs, clarified sirup of sugar, one hundred and twenty-eight parts, medicinal hydrocyanic acid, one part. It is perceived, therefore, that while the acid constitutes a tenth part of the sirup of the Codex, it forms only the one-one hundred and twenty-ninth part of that of MAGENDIE. Every half ounce of the Codex sirup contains 57.6 grains of medicinal hydrocyanic acid, while the same quantity of MAGENDIE's sirup contains only 2.24 grains; so that it appears that the unfortunate patients of Dr FERRUS took about twenty-five times as much medicinal hydrocyanic acid at a dose, as he had intended to give them.

Dr GENDRIN proceeds with some criticisms on the formulæ for hydrocyanic acid of the French Codex. It appears that the hydrocyanic acid of this official pharmacopœia is prepared according to two formulæ, one being designated as that of ROBIQUET, the other as derived from VAUQUELIN. But Dr GENDRIN affirms that ROBIQUET's formula is not correctly followed: for while he directs the anhydrous acid to be diluted with *twice* its volume of water, the Codex directs it to be mixed with its own volume! But even ROBIQUET's acid is stronger than that of MAGENDIE, which consists of one measure of acid to six of water.

Supposing the syrup of the Codex prepared with the strong acid, erroneously said to be according to the formula of ROBIQUET, half an ounce of it would contain nearly twelve grains of pure anhydrous hydrocyanic acid. It may be easily imagined what would be the action of such a dose!

Dr GENDRIN states, however, that the acid obtained for administration at the Bicêtre was not so strong as that above indicated. The pupil at the laboratory of the central pharmacy prepared it according to a formula, which had recently been published in the Journal of Pharmacy. By it, he obtained an acid stronger than 0.90, the density directed in the Codex. He accordingly diluted it with water, and for the sake of greater precaution, brought it to 0.92, or weaker than directed by the Codex. It was with an acid of this strength that the fatal sirup had been made. Every half ounce of it contained nearly eight grains of pure acid.

Dr GENDRIN next mentions the other formula of the Codex, or that of VAUQUELIN, which contains about 21 parts of pure acid to 800 of water, and which is, therefore, weaker than MAGENDIE's acid, and consequently still more so than that of ROBIQUET. Dr GENDRIN speaks in terms of disapprobation of the framers of the French Codex, for having so carelessly and unwarrantably directed the same medicinal article to be prepared by different formulæ, which produce it of different strengths; for, says he, a patient who could take a given dose of VAUQUELIN's acid without danger, would be infallibly destroyed by the same dose of acid of the density of 0.90, or even 0.95, or by half the quantity, when prepared by the Codex formula, erroneously attributed to ROBIQUET.

Here perhaps Dr GENDRIN goes too far in his strictures on the Paris Codex. We have not the work before us, but it seems probable, from what Dr G. himself states, that the Codex, in directing a particular specific gravity (0.90), had in view that the acid should always be brought to that strength. Hence, though there are different formulæ laid down, which furnish products of different strengths, yet the acid is not the acid of the Codex, unless it have the specific gravity of 0.90. This difficulty, however, occurs, that VAUQUELIN's acid weighs as heavy as 0.98: now it may be very easy to dilute the acid if too strong, but it is not so easy to abstract a portion of water, in case it should be too weak.

Dr GENDRIN sums up his criticisms in the following words. "Thus in whatever point of view we consider the formulæ of the Codex, they are in all respects erroneous. They are erroneous in the density stated of the acid of SCHEELE; erroneous in the preparation of the hydrocyanic acid of ROBIQUET, in ordering but half the water directed by this pharmaceutist; erroneous in assimilating the

acid of ROBIQUET to that of SCHEELE, which is three times as weak, and to that of VAUQUELIN, which is still weaker; and finally, erroneous in directing a quantity of acid in the cyanic sirup beyond all reasonable proportion."

Dr GENDRIN gives an extract from the formulary of MAGENDIE, by which it appears that this distinguished physician pointed out the dangers of these formulæ of the Codex, and even foresaw the disastrous consequences likely to ensue from a preparation of such inordinate strength as the cyanic sirup. His words are, "This sirup, thus prepared, could only be exhibited by drops; for if, by any mishap, an ounce was mixed in any potion, it would prove certainly mortal."

The bodies of the seven unfortunate patients were opened by Drs ADELON and MARJOLIN. They did not find any lesion which could be assigned as the cause of the epilepsy. The heart was empty, and the blood in the vessels not coagulated. This liquid was found congested in the vessels of the brain and abdomen. The bodies did not exhale any odour of bitter almonds.

55. *State of Pharmacy in France.*—The great attention which is paid to the instruction of pupils in the science of pharmacy in France may be learned from the following account of an examination of candidates, for the places of pupils in pharmacy to the hospitals of Paris, which we condense from the Journal de Chimie Médicale for July 1828.

Count CHAPTAL presided at the examination, assisted by Drs JADIAUX and LALLEMANT, M. HENRY, chief of the central pharmacy, and MM. DUVAL, HARVENG and PETROZ, chief pharmaceutists of the hospitals. The exercises consisted of three kinds: 1st. Questions to which written answers were required; 2d. Questions to be answered verbally; and 3d. Manipulations, which were performed before the board of examiners. The questions to be answered in writing were, 1st. What is gum arabic? describe the kinds in commerce and state their origin. 2d. State the preparation of citrin ointment, its characters, and the alterations which it is liable to undergo by time, &c. 3d. What is the chemical composition of opium? give the methods of extracting morphia and narcotin. The verbal questions related to the different kinds of distillation;—the nature of sirups and their mode of preparation;—the menstrua for separating the soluble parts of plants;—the nature of conserves, and the general rules of their preparation;—the distinctions between cerates, pomatums, and ointments, and the general rules for their preparation. The manipulations required to be performed were 1st. To make an emulsion with turpentine;—2d. To prepare whey.

The examination having terminated, the written answers of the candidates were considered at seven meetings. The relative merits of the pupils were discussed by the aid of the notes taken by the examiners, and each was classed according to his proficiency.

Finally, the board of examiners, at a public sitting, announced the names of those candidates who were deemed worthy of being recommended as resident pupils, and of those also, who were to be considered as provisional pupils, to fill vacancies until the next examination.

A course of examinations of this kind, in which, not merely the absolute, but the relative acquirements of the candidates are ascertained, must be viewed as well calculated to call forth the greatest exertions on the part of the pupils, on the principle of rivalship. It deserves to be imitated, as far as possible, in the schools in which pharmacy is taught in the United States.

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